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EDITED BY

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CHANGES IN MUSCULAR TENSION IN CO-ÖRDINATED HAND MOVEMENTS

BY BUFORD JOHNSON

The Johns Hopkins University

Observations of children four and five years of age in attempts to coördinate arm, hand and finger movements into specific patterns suggested the importance of differences in the muscular tension during the performance. These differences were indicated by the form of grasping, by the pressure exerted by the hand not directly engaged in the performance and often by facial contractions. The problem of measuring the degree of muscular tension during such performances as tapping, writing and maintaining a steady hand and of relating the data obtained to other measurements that indicate emotional tendencies was attacked. The major problem was the designing and constructing of apparatus that would give the desired results. This paper reports a preliminary study of the pressure exerted in eye-hand coördinations with a description of the apparatus.

The relative amount and the changes of pressure exerted by the fingers in grasping a stylus during the performance of tapping upon a board as rapidly as possible were studied. The tapping board used is of the type previously described by Dunlap in 1921.[1] The writer reported results from performances of children on this board in *Mental Growth of Children*, 1924. The board consisted of two brass plates 3 inches square mounted on a heavy base separated by a bakelite block one inch wide, which rises 3/16 inch above the surface of the plates. Instead of the ordinary tapping stylus, the one used was made of a rubber tube fitted to brass disks at either end connected by a small brass rod. The rubber was of approximately the same elasticity as that of a light automobile tube. Through one of the disks the brass rod was extended to the length of the usual stylus for tapping upon a plate; to the other disk was fitted a rubber tube leading to a tambour by which a kymographic record of the changes of pressure

[1] K. Dunlap, Improved forms of steadiness tester and tapping plate, this JOURNAL, 1921, 4, 430–433.

RECORDS OF ADULTS

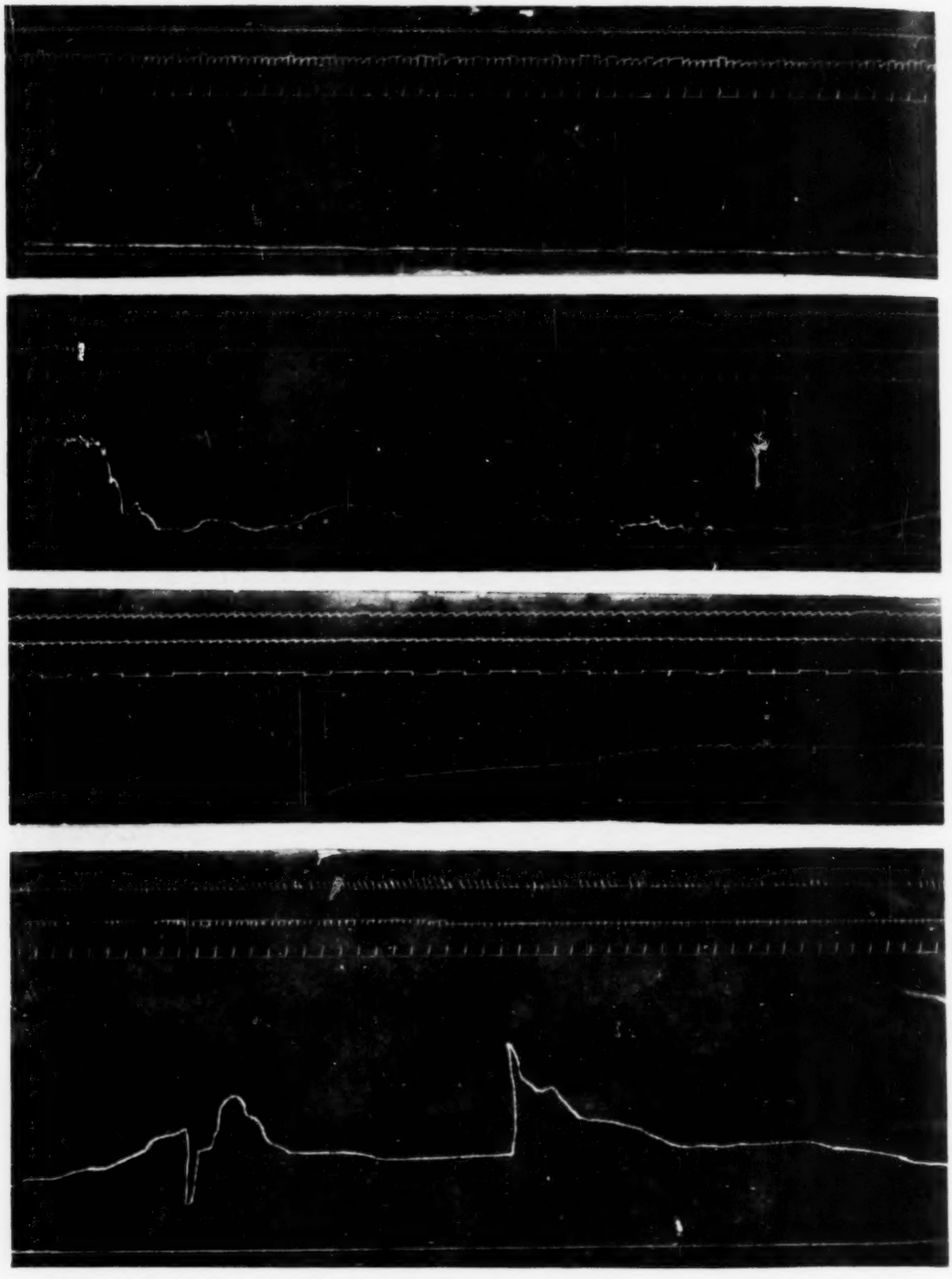


FIG. 1

RECORDS OF CHILDREN

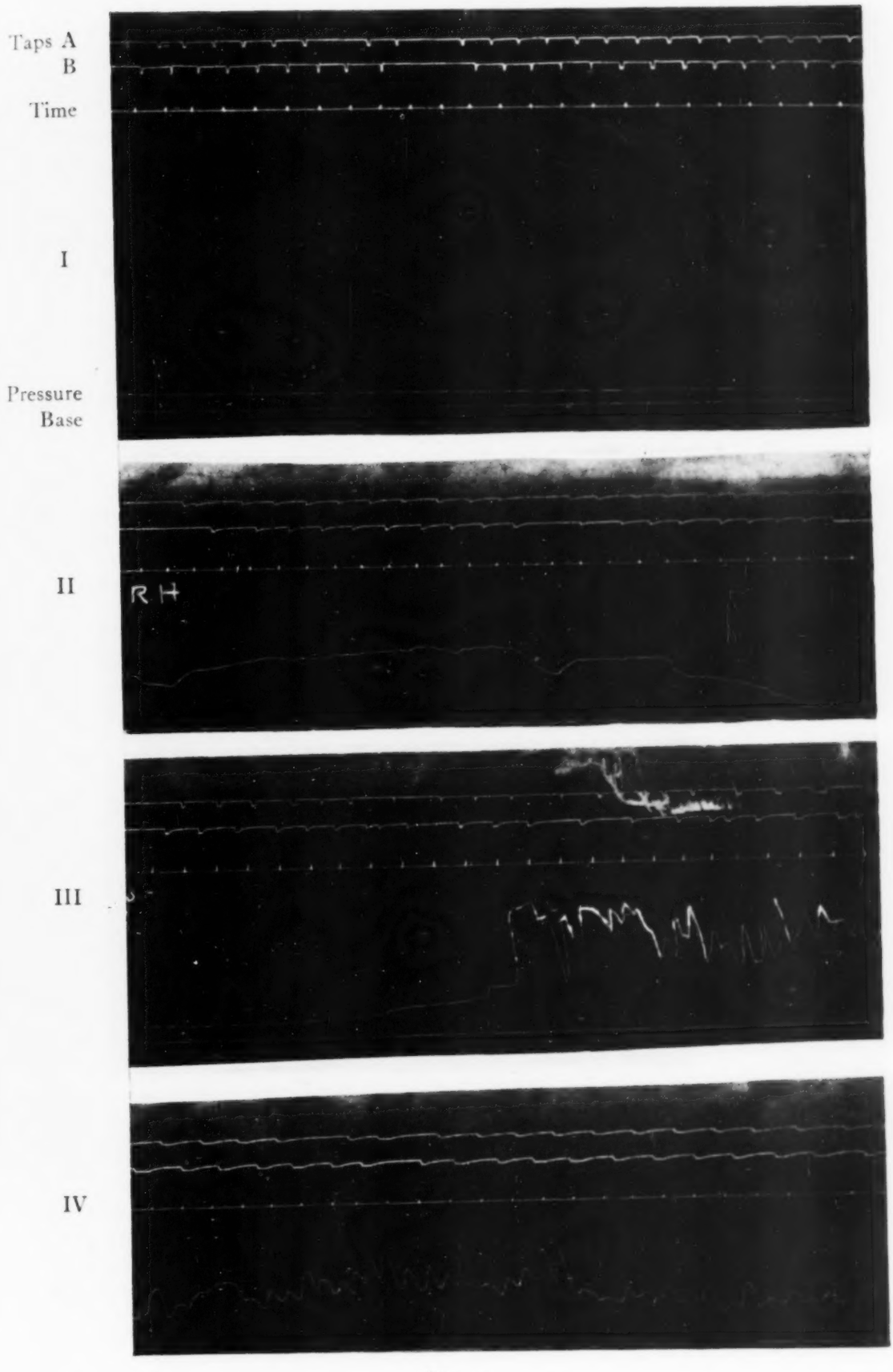


FIG. 2

upon the stylus was made. The tapping board and stylus were wired in series with a triple marker so that records of the number of taps made on each plate and of the changes in pressure were registered by the kymograph. A Jacquet chronometer was used to record time in seconds. The reactors stood during the performance, which consisted in tapping first upon one plate and then upon the other for a period of one minute. The right hand was used first and after an interval sufficient for adjusting the drum the left hand was used in a similar manner. The recording apparatus was screened from view.

The rubber stylus used in this study must be held in the central portion, *i.e.* within a zone that is equidistant from either end in order to get comparable records. Otherwise, a grasp near the end results in a record of less pressure as the brass disks act as supports. For the tapping movement the small rubber tube is somewhat more of a cumbrance than the ordinary wire, hence the records for speed should not be compared with tapping records made by the usual method. For comparisons within the group tested, under the conditions stated, the records obtained appear to give valid results. Illustrations of individual differences in pressure exerted by children and adults are given in Figs. 1 and 2.

The treatment of the pressure record in quantitative terms was made by the following method of scoring. A stencil of celluloid was so made that it would fit a section of the record corresponding to a 15-sec interval of tapping. Fifteen parallel vertical lines were drawn at a distance from each other equal to that between second-

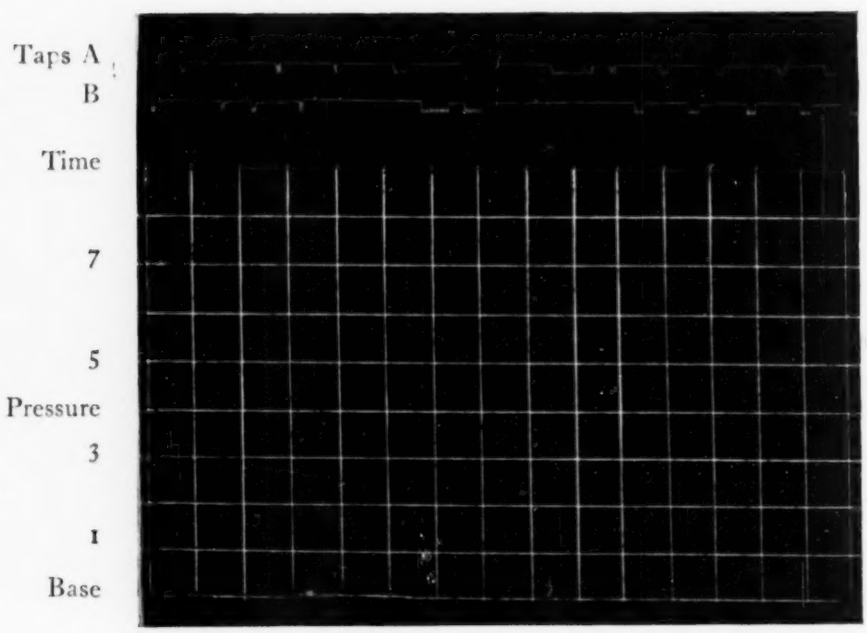


FIG. 3

marks on the time record. The height of the stencil was such that these lines would extend from the time record to the base or zero line of the pressure record. Ten horizontal parallel lines were drawn at the same distance apart as were the vertical, thus forming a coördinated series of lines spaced according to the second-interval. The vertical columns were numbered from one upward beginning at the bottom. The stencil is placed over the kymographic record with the bottom horizontal line

fitted over the base line for the pressure record, and the first vertical line at the left coinciding with the first second-mark of the time record for the unit to be scored. A score is then recorded for the height of pressure in an interval of one second in terms of distance above the base line for pressure. The greatest height attained in the column formed by the vertical lines is the score used for that interval. The pressure line for a

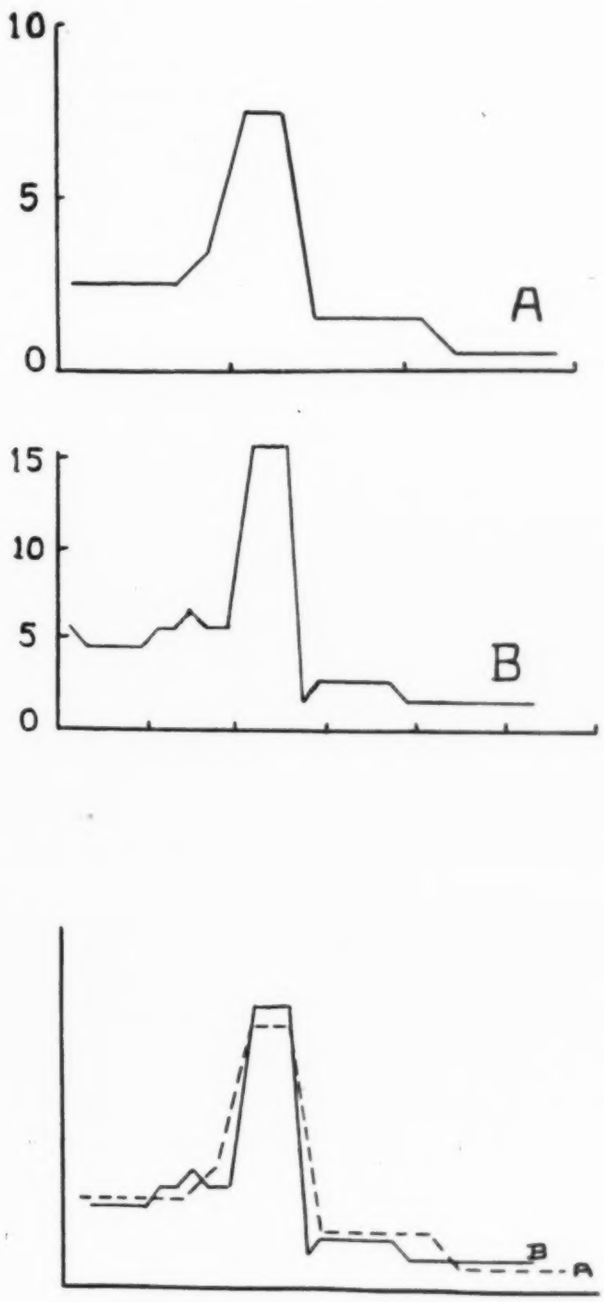


FIG. 4

given interval must cross the upper boundary of the square before the score of the upper square is recorded. To illustrate this method the pressure line for the eighth column in Fig. 3 touches the upper boundary of the horizontal row scored 1, but does not cross

into row 2. The score recorded would be 1. In the first column the score is 5. One unit of a pressure record as scored for each second-interval was as follows; 3, 3, 3, 3, 4, 8, 8, 2, 2, 2, 2, 1, 1, 1, 1. The curve as plotted from these scores is shown in A, Fig. 4. This gives a fairly adequate picture of the changes in pressure recorded. A further analysis of these scores was made by reducing the squares of the coördinate series to one-half the original size. The unit for such divisions is one-half the interval between the second-marks. The scores by this scale were as follows; 6, 5, 5, 5, 5, 5, 6, 6, 7, 6, 6, 16, 16, 16, 2, 3, 3, 3, 3, 3, 2, 2, 2, 2, 2, 2, 2, 2. The curve plotted from these scores is B in Fig. 4. These two curves are also shown superposed. The finer unit of scoring does not seem warranted for these records. A study of a wider range of individuals may show the need for the smaller interval for scoring.

The analysis of the records for 5 adults and for 9 children has been made with reference to speed and accuracy of movement; to right-hand and left-hand performance; and to such a complication of performance as the ordinary simple tapping movement in comparison with the alternate plate performance. Additional records for 15 other reactors, 6 adults and 9 children, show the same types of pressure-records as those scored for analysis. In Tables I, II and III are given the scores in terms of number of taps made on each plate by each hand and of the average height of the pressure curve for a 15-sec interval in units of the interval between second-marks.

Types of Pressure Record

The adult records. These show distinct individual differences in amount of pressure exerted by the fingers and also in changes of pressure throughout the performance. These adults were graduate students familiar with the tapping test and informed as to the theory that temperamental differences are indicated by differences in tension of expressive muscles under differing stimuli and that the hand shares with the vocal muscles in frequency and intensity of contraction under emotional stimulation. The individual records of adults illustrate four types of pressure records;

(I) Slight pressure maintained throughout the performance
(II) Initial rise in pressure with gradual decrease
(III) Slight pressure in beginning with gradual increase, and
(IV) Irregular changes and (on the average) a high pressure throughout the performance.

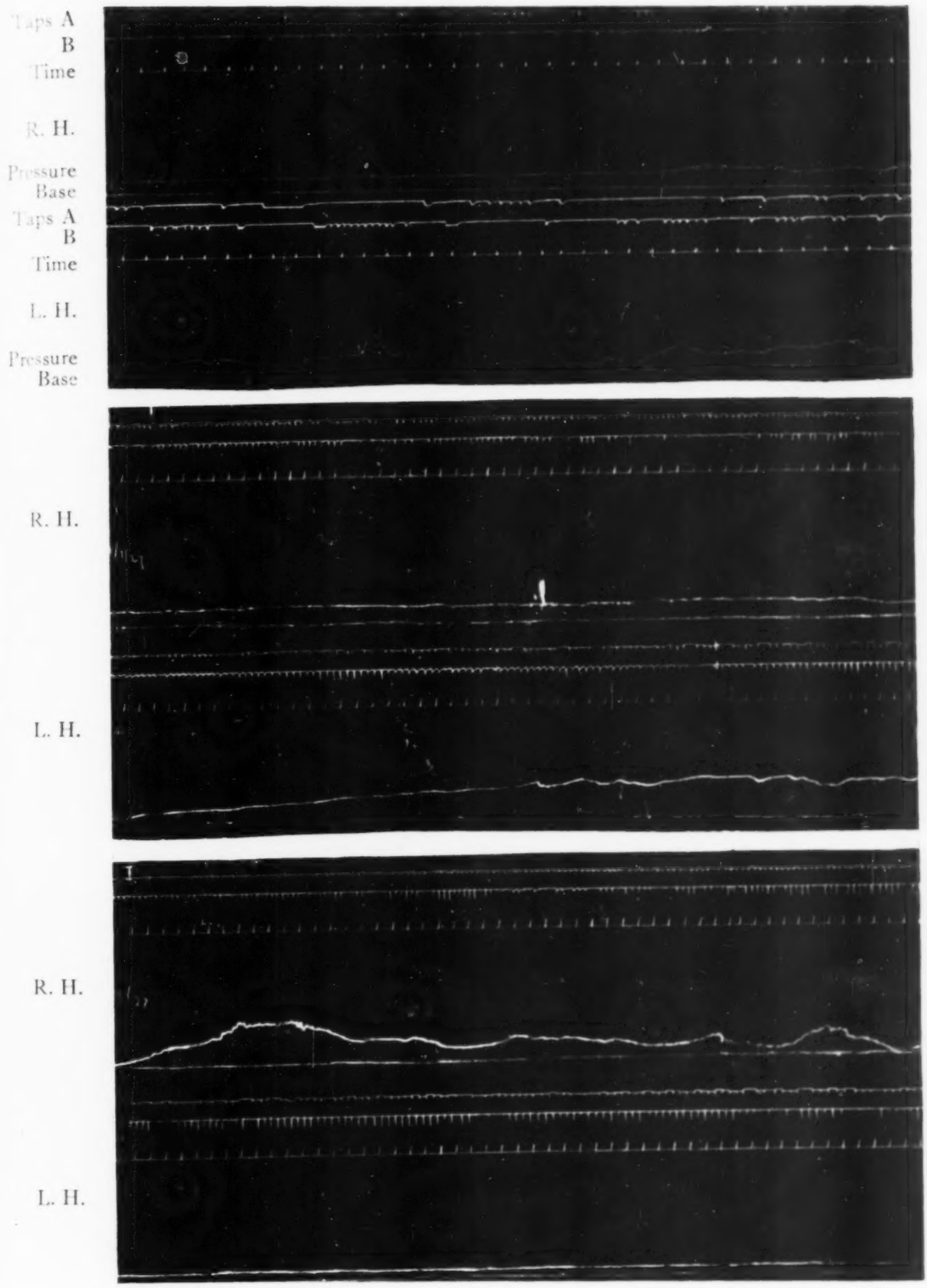

Taps A
B
Time
R. H.
Pressure
Base
Taps A
B
Time
L. H.
Pressure
Base
R. H.
L. H.
R. H.
L. H.

FIG. 5

A study of the efficiency of the performance, as indicated by speed and precision of movements, shows that reactor J, who used the lightest grasp and maintained it throughout, was the most efficient. Type I is illustrated by this record

Table I

SCORES OF ADULTS IN ALTERNATE TAPPING

Interval	Right Hand T	A	B	D	P	Interval	Left Hand T	A	B	D	P
					Reactor J (Man)						
15 sec						15 sec					
(1).....	89	45	44	1	1	(1).....	66	33	33	0	1
(2).....	76	39	37	2	1	(2).....	59	30	29	1	1
(3).....	73	36	37	−1	1	(3).....	58	29	29	0	1
(4).....	71	35	36	−1	1	(4).....	56	29	27	2	1
Total....	309	155	154			Total....	239	121	118		
					Reactor H (Woman)						
15 sec						15 sec					
(1).....	78	41	37	4	1.67	(1).....	58	23	35	12	1
(2).....	82	42	40	2	2.33	(2).....	58	28	30	−2	1
(3).....	74	41	33	8	1.87	(3).....	65	32	33	−1	1
(4).....	72	38	34	4	1.6	(4).....	50	25	25	0	1
Total....	306	162	144			Total....	231	108	123		
					Reactor D (Woman)						
15 sec						15 sec					
(1).....	85	41	44	−3	4.2	(1).....	69	35	34	1	4
(2).....	71	35	36	−1	2.4	(2).....	57	28	29	−1	2.53
(3).....	58	30	28	2	2.2	(3).....	51	23	28	−5	2.23
(4).....	62	32	30	2	1.6	(4).....	64	34	30	4	2.33
Total....	276	138	138			Total....	241	120	121		
					Reactor F (Woman)						
15 sec						15 sec					
(1).....	68	40	28	12	1.07	(1).....	75	36	39	3	1
(2).....	62	31	31	0	1.87	(2).....	64	30	34	4	1.93
(3).....	62	34	28	6	1.33	(3).....	59	26	33	−7	2.87
(4).....	59	37	22	15	1.67	(4).....	53	21	32	−11	3
Total....	251	142	109			Total....	251	113	138		
					Reactor C (Man)						
15 sec											
(1).....	89	45	44	1	6.2						
(2).....	77	37	40	3	6.4						
(3).....	76	37	39	2	6.47						
Total....	242	119	123								

A = Number of taps on plate at right of board; B = number of taps on plate at left of board; T = total number of taps for the two plates; D = difference between A and B, and P = average pressure for 15-sec interval.

of J. Speed is measured by the number of taps in a given period: precision is measured by the accuracy in following

the instruction to strike first upon the one plate and then upon the other. A difference of one between the number of taps for the two plates may be expected from the division into these units; but a greater difference indicates either a failure to reach the second plate, the tap falling upon the dividing board or else just missing the plate, thus shortening the stroke, or the omission of the movement toward the second plate and a repetition of the tap on the first. If large units of a record are used for scoring, these differences may equalize each other to such an extent that the incoördination is not recognized. Scores for J and D (Table I) with those of H and F illustrate this fact. The total number of taps in 60 sec for J (right hand) was 309, which was the highest score made; his score for the left hand was 239, the third in rank. The two reactors, D and F, who excelled J with the left hand, were, however, less precise, and a comparison of the total number of taps in this 60-sec period without a penalty for inaccuracy is not a reliable measure of efficiency.

The highest score for the left hand made by F was 251; but the differences for the two plates ranged from 3 to 11 for 15-sec intervals, with a total of 25 for the minute period. Plate A (the plate at the right of the board) received more taps during each interval of 15 sec, suggesting a constant lack of accuracy in coördinating the tapping movement required. A large difference of 33 was shown for the same reactor F in right-hand performance; but even with this inaccuracy favoring the speed she ranked lowest in speed. The reactor D, who excelled J by only 2 taps, had a difference of 11 taps between the two plates as compared with a difference of 3 taps for J.

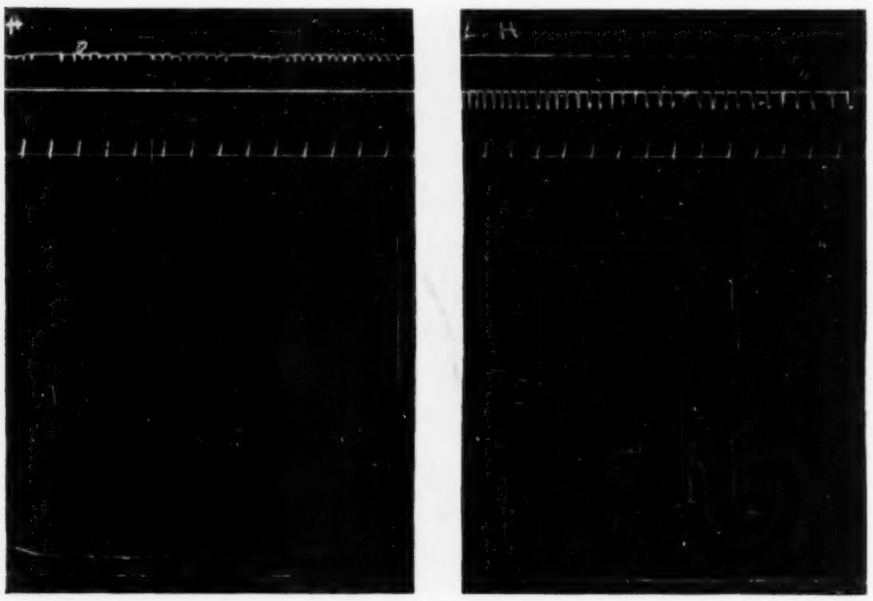
Reactor D was the next most efficient performer. Her pressure record (Type II) is that which shows a marked height at the beginning which gradually decreases, though it never becomes a relatively low pressure. A high degree of accuracy in movement is shown for most intervals; though a tendency to become more incoördinated with continued performance is indicated.

Type III, the increasing-pressure record, was made by

reactor F, whose scores as discussed above show a failure to attain precision of movement. It is possible that speed was the dominant motive in the performance. Whether failure to coördinate accurately will generally result in increasing the tension of the fingers or whether speed as a primary aim will result in lack of accuracy and greater tension under con-

SIMPLE TAPTING

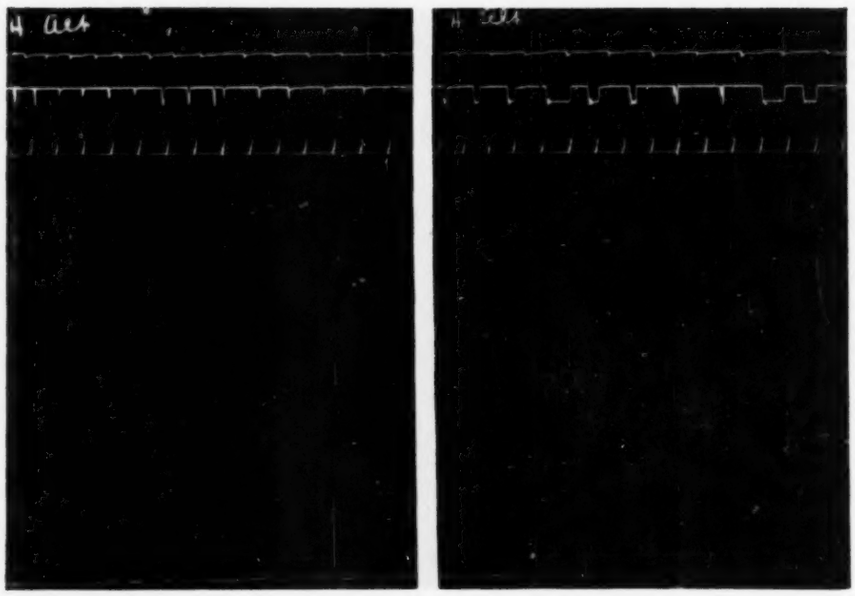
R. H.



L. H.

ALTERNATE TA. PING

R. H.



L. H.

FIG. 6

tinued performance cannot be stated without analysis of many more records. It is interesting to find that H, who also shows a lack of accuracy, tends to increase the tension with the right hand but maintains a fairly constant low pressure with the left. The irregularity of her performance is slightly less and the total output considerably less for the left than for the right hand.

Type IV, the high pressure record with erratic changes, was made by subject C, whose record in Table I is incomplete because of accidental blurring. His speed record during these intervals of .15 sec each is greater and the accuracy is but little less than that for J, who makes the highest score. This type suggests the individual who works under high tension but with high efficiency for limited intervals of time.

Records of children. The records made by children between 2 and 6 years show as striking differences in the pressure of the same child for different types of performance as do the various individuals among themselves. Types I, II, III and IV, as made by adults, are also found among the children's records. A child's right-hand performance may be of one type, however, with a distinctly different type for the left.

The tension in grasp for some of the children also varies with the degree of complication in the movements. The children were given two series of trials with an interval of 3 to 4 weeks. One series consisted of a right- and left-hand performance in simple and in complicated tapping. A brief interval was required for changing one performance to the other and for marking the change on record-paper. Four types of performances were recorded on the same paper and at one sitting. A simple tapping

TABLE II

SCORES OF CHILDREN IN SIMPLE TAPPING

		Right		Left		Right		Left	
Subject	C.A.	T.	P.	T.	P.	T.	P.	T.	P.
L.R. (girl)	2.7	27	3	24	2.5	24	1.07	16	1
N.G. (boy)	2.10	34	1	35	1	42	1	38	1
G.R. (boy)	3.4	31	4	41	4	43	1	29	1.53
D.E. (girl)	3.8	34	1	37	4.13				
N.J. (girl)	3.11	37	2	34	3				
R.V. (boy)	4.2	39	1	36	1	53	1	45	1
M.P. (girl)	4.3	41	3	43	6.6	60	1.13	30	4.13
M.J. (girl)	4.10	40	1.07	34	2.93	32	1	27	1
D.R. (boy)	5.4	37	1	38	1	39	1.01	40	1

C.A. = chronological age; T = number of taps; P = average pressure.

movement for 15 sec on Plate A for the right hand and Plate B for the left was the first type of movement recorded. After instruction as to the more complicated movement of tapping first upon Plate A and then upon Plate B and a trial performance of

TABLE III

SCORES OF CHILDREN IN COMPLICATED TAPPING

	(1) Right					(1) Left					(2) Right					(2) Left				
	T	A	B	D	P	T	A	B	D	P	T	A	B	D	P	T	A	B	D	P
L.R.....											24	12	12	0	1	18	7	11	4	1.53
N.G.....	11	5	6	1	1	14	7	7	0	1	17	9	8	1	1	18	9	9	0	1
G.R.....	23	15	8	7	3.4	28	14	14	0	4.26	24	12	12	0	1.27	18	6	12	6	1.67
D.E.....	33	14	19	5	1.4	29	16	13	3	4.13										
N.J.....	28	14	14	0	4.4	24	13	11	2	2										
R.V.....	21	11	10	1	1	24	12	12	0	1	25	13	12	1	1.07	25	13	12	1	1.20
M.P.....	27	14	13	1	1.07	23	12	11	1	1	19	18	1	17	1	21	13	8	5	4.2
M.J.....	27	13	14	1	1	20	10	10	0	1	34	17	17	0	1.07	26	13	13	0	1
D.R.....	34	17	17	0	1	31	16	15	1	1	43	21	22	1	1.01	35	18	17	1	1.2

T = number of taps; A = number of taps on plate at right of board; B = number of taps on plate at left; D = difference between A and B, and P = average pressure for a 15-sec interval.

this movement, a 15-sec period of tapping with the right hand was given and followed by a similar left-hand performance. The contrast between the efficiency in simple and in complicated tapping is illustrated in Fig. 6. Some children show a tendency to use less pressure in the more complicated movement. The output is always less for this type of performance than for the simple tapping; but in the case of some children it is greatly reduced. It may be that the attention is directed more toward speed or toward manipulation of the stylus. Coördination primarily through visual guidance may be the case in general in the complicated type of tapping while kinæsthetic guidance may determine the simple form. Individuals would probably vary in the extent to which attention was directed either toward manipulation of the stylus or toward the object to be tapped. The effect of the direction of the attention would partially explain also the differences in performance of the two hands. When such use of the left hand is a much more unusual activity than that of the right, the attention in tapping may be centered upon the movement of the hand and this direction of attention would probably result in an increased tension there. The emotional tendencies of the individual would determine to a greater extent the type of pressure-record for a new or difficult performance than for an automatic activity which was accompanied by but little excitement.

From this preliminary study only tentative conclusions as to individual differences in muscular tension can be made. Adults and children show wide differences in muscular tension of the fingers in the tapping movements. When speed is measured by the number of taps made in a given period and precision of movement is measured by accuracy in following the instruction to strike first upon one plate and then upon

another, efficiency of performance appears to be directly related to tension with certain limitations of time. For a period of one minute the reactor who maintains light pressure throughout is the most efficient; the next best is the reactor whose tension is high in the initial stage but gradually decreases. Reactors who show the other two types of curve (gradual increase in tension or high pressure throughout) may be rapid and precise for a brief initial period but they slow up and tend to become inaccurate in the later stages.

The individual who does not have a well-established habit of efficient expression by the hand appears to be easily disintegrated and he appears to exert unnecessary effort in attempts to make efficient responses. The differences in tension between individuals at the same level of practice for the specific performance suggest that this measurement may contribute to the study of individual differences in the fundamental tendencies often called temperamental traits.

(Manuscript received March 9, 1928)

THE RELATION BETWEEN 'INTELLIGENCE' AND REFLEX CONDUCTION RATE

BY LEE EDWARD TRAVIS AND THEODORE A. HUNTER

University of Iowa[1]

A factor in mental ability which has received considerable attention is speed. Peak and Boring (1) made a study of speed in intelligence and found a high positive correlation (.90) between scores (standard time) of both Alpha and Otis tests and reaction times. They concluded that "speed of reaction is an important, and probably the most important, factor in individual differences in the intelligent act." Although they studied only five subjects their results indicate the desirability of similar attacks upon the same problem. Farnsworth, Seashore and Tinker (2), moved by the results of Peak and Boring, made a more extensive study which included serial action and more difficult tests of intelligence. They came to the general conclusion that simple reaction time has a slightly negative correlation with the Otis advanced, Army Alpha, Thorndike and Ohio State, Part 5; while serial action has an appreciable correlation with the intelligence tests used. This conclusion is in harmony with most previous studies, the results of Peak and Boring being the main exception.[2] The most recent study of speed in mental reactions which has come to our attention was made by Rounds (3). He reports a correlation of .59 between the total raw score on a battery of mental tests and the latent time in the Achilles tendon reflex. The mental tests were the first addition test in the Courtis series, and letter cross-out, figure cross-out, completion, easy word-object association, easy word-opposite association, and Taylor recognition tests. All of these tests except the Taylor are

[1] A preliminary report of this study was presented before the National Academy of Sciences at Urbana, Illinois, October 18, 1927, and printed in *Science*, 1928, 67, 41-43 (Jan. 13).

[2] Farnsworth, Seashore and Tinker give a good historical survey of earlier studies.

in common use. Eighty subjects, the great majority of them Columbia College students, served in Rounds' study. Rounds designated Achilles tendon reflex-time to be the time interval between the instant of stimulation of the tendon and the thickening of the executant muscle.

In certain studies of the knee-jerk, which took into consideration elapsed time between stimulation of the patellar tendon and the appearance of action currents in the executant muscle, we noted an apparent relationship between this reflex-time and intelligence. Verification of this observation constitutes the basis of the present paper.

Apparatus and Method

Muscular contraction is accompanied by changes in electrical potential which are commonly called action currents.[3] Since these electrical impulses are extremely small it is necessary in the study of them to use an exceptionally sensitive recording device or some means of amplification with a less sensitive recording instrument. From our experience it seems preferable to use a medium amount of amplification with a moderately sensitive recording device.

We have passed through several stages in the development of our apparatus and some of the data were obtained with two different amplifying-recording units. Our purpose was not to study wave form but only the time of incidence of action currents in the quadriceps femoris muscle and both sets have proved adequate. The apparatus finally developed for our studies and the one which we shall describe may be used, however, for studying wave forms and periodicities.

The amplifier is a three-stage resistance coupled unit. Fig. 1 shows the circuit diagram. Transformer T_1, a Western Electric 226 A input transformer, has a center-tapped primary which is used in order to eliminate, partially at least, the effects of

[3] Action currents from skeletal muscles in voluntary contraction were recorded for the first time by Piper (*Arch. f. d. ges. Physiol.*, 1907, 119, 301). Snyder (*Amer. J. Physiol.*, 1910, 26, 474) and Hoffman (*Arch. f. d. ges. Physiol.*, 1910, 223) were the first to study the latent time of the knee-jerk by means of recording electrical changes in the executant muscle. Forbes and Thacher (*Amer. J. Physiol.*, 1920, 52, 409) reported the first use of vacuum tube amplification in the study of action currents. Recently Adrian and Zottermann (*J. Physiol.*, 1926, 61, 151) have devised an excellent method for studying electrical changes in nerves and muscles.

stray electro-magnetic and electro-static fields such as those arising from 60-cycle alternating currents, and at the same time not to reduce the amplification of the voltages coming from the contracting muscle. This result is accomplished by the

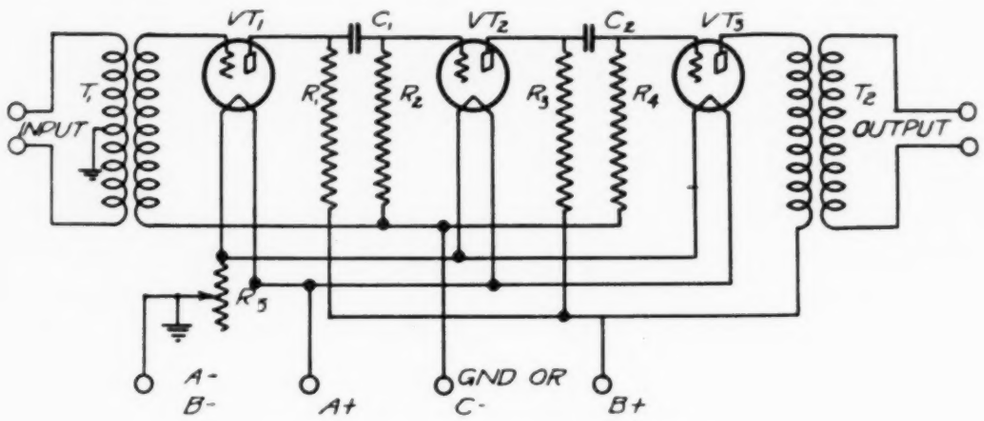


FIG. 1. Circuit diagram of amplifier.

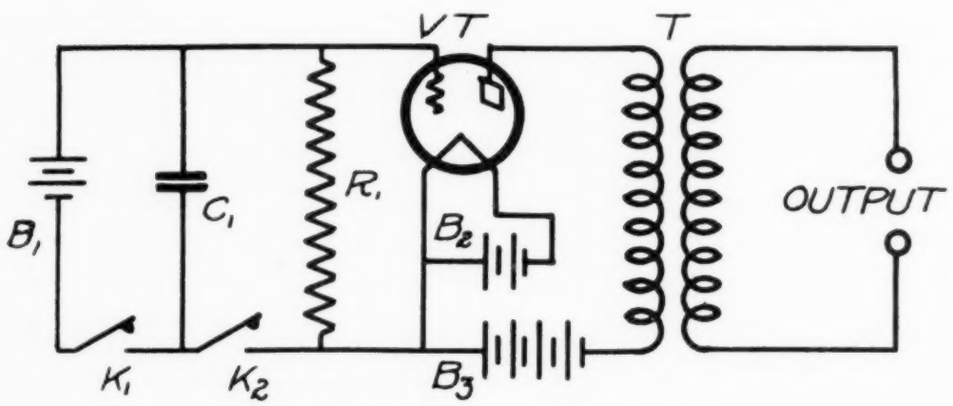


FIG. 2. Wiring diagram of signal circuit.

balancing effect of the two halves of the transformer. The vacuum tubes VT_1, VT_2, and VT_3, may be either of the P J 11 or the U X 240 type. These two kinds of tubes are identical in structure, each having an amplification factor of approximately 30. The U X 240 tube is more easily obtained, being an item of quantity production from the factory. We used the P J 11, however, because it has less tube noise. Resistances R_1 and R_3 are similar, each having approximately 250,000 ohms. Resistances R_2 and R_4 are similar also, each having 3 megohms. Condensers C_1 and C_2 each have a capacity of .01 micro-farads. The output transformer T_2 is specially designed, consisting of approximately 80 turns of size 18 double cotton-covered (D. C. C.) Brown and Sharp gauge (B & S) copper magnet wire which is wound on an Amertran type 854 choke coil. This transformer changes the low current of high voltage into a high current of low voltage to match the impedance of the oscillograph element. The filament rheostat R_5 has a resistance value of six ohms.

The "A" battery supply is 6 volts, the "B" battery 180 volts, and the "C" battery $1\frac{1}{2}$ volts. By connecting the grid return to the A– B– terminal, the "C" battery may be dispensed with. The brass panel of the amplifier is used as a common ground. The amplifier magnifies the action currents between 500,000 and 800,000 times.

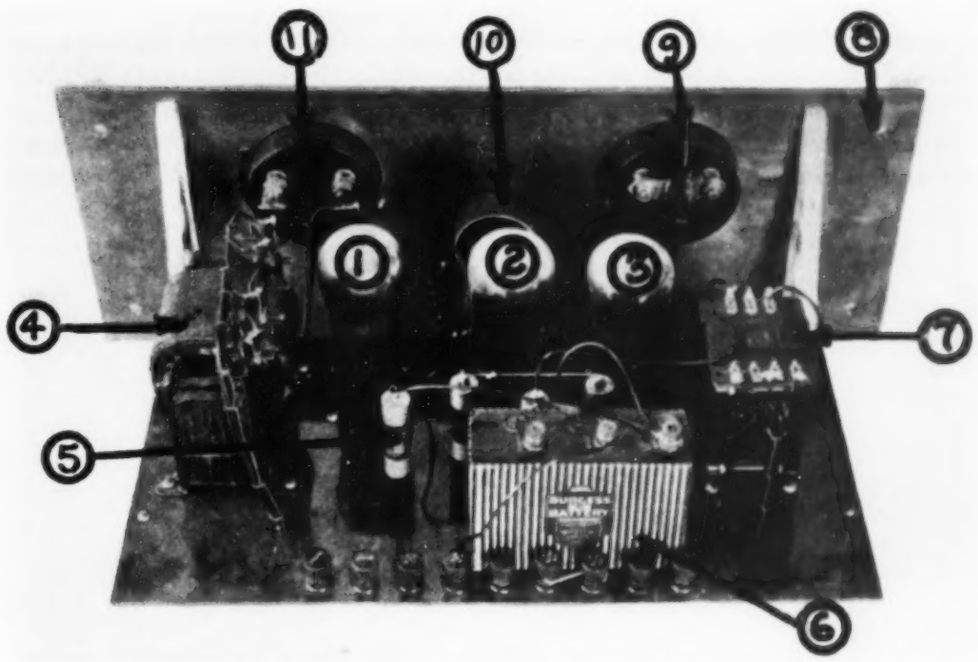


FIG. 3. Top view of amplifier. 1, 2, and 3 = P J 11 vacuum tubes; 4 = output transformer; 5 = resistance (R_4 in text) which is similar to the other three resistances to the right; 6 = "C" battery; 7 = input transformer; 8 = brass panel; 9 = milliameter; 10 = filament rheostat; and 11 = filament voltmeter.

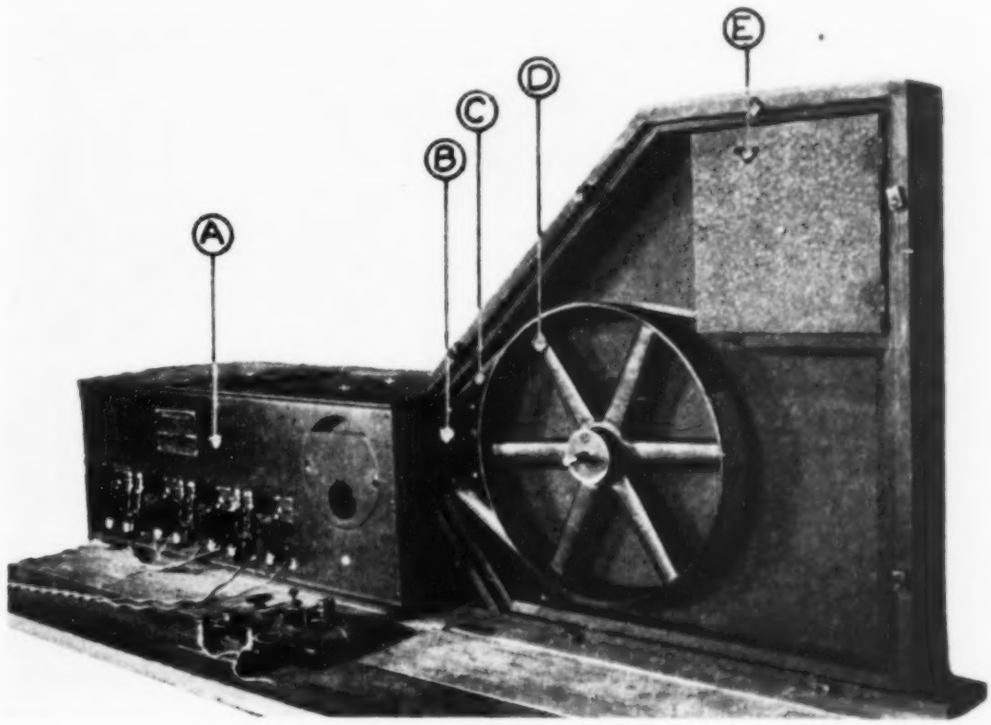


FIG. 4. Side view of photographic unit with cover removed. A = oscillograph; B = film guide; C = film; D = film wheel; and E = 400-foot film holder (Eastman Kodak Company).

Our recording instrument is known as a portable, three-element oscillograph manufactured by Westinghouse. One element was used to record action currents, another the time-line, and the third the instant of stimulation of the patellar tendon. The element recording action currents is a supersensitive one, being approximately five times as sensitive as the other two. All elements have a very high natural period of vibration and are immersed in a dampening fluid. They are excelled in promptness

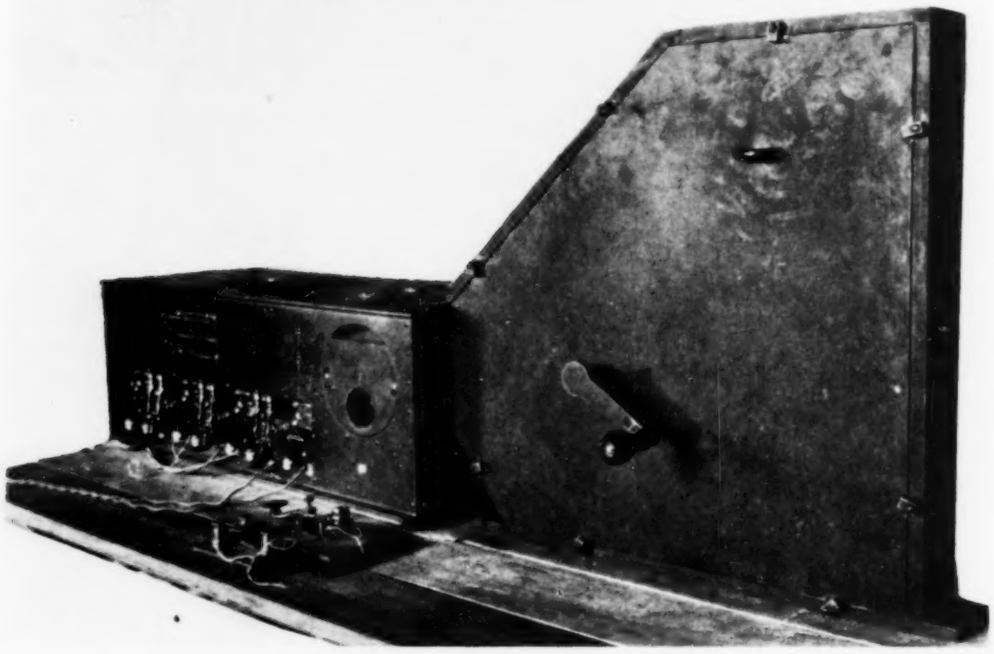


FIG. 5. Side view of photographic unit with cover in place.

of response only by the cathode ray oscillograph. We devised a special photographic unit to replace the one with which the oscillograph was originally equipped. This specially built unit carries four-hundred feet of standard-size moving picture film. It requires about $3\frac{1}{2}$ feet of film for one record. For easy reading the film should travel at the rate of at least 2.2 feet per second.

It is necessary to use a special signal circuit because of the sensitiveness of the amplifier to the pulsating and alternating electrical fields which are generated by the commonly used signalling methods. Figure 2 shows a wiring diagram of our signal circuit. The main object is greatly to reduce the amount of power consumed in the circuit. This is accomplished by charging a .1 microfarad fixed condenser C_1 with the potential of $1\frac{1}{2}$ volts (B_1), by making contact at K_1. Then when the contact K_2 is made at the patellar tendon the small amount of power previously stored in condenser C_1 discharges through a resistance (R_1) of 3,000 ohms, causing a change in electrical potential on the grid of a U X 199 vacuum tube VT. This change in potential on the grid causes a change in the plate circuit which passes through the primary of the output transformer T. Transformer T is of the same type and accomplishes the same results as T_2 in Figure 1. B_2 is three volts and B_3 forty-five. A General Radio low frequency oscillator Type 377 which is capable of producing frequencies varying from 60 to 80,000 complete cycles per second is used to furnish the time-line. We used a frequency of 1,000 complete cycles per second because of ease and reliability

of reading our values in terms of .001-sec intervals. The output of the oscillator is connected directly to the input of the oscillograph element.

The electrodes were German silver discs 27 mm in diameter, covered with canton flannel which was soaked before each experiment in a saturated saline solution. More recently we have been using as electrodes thin brass strips ½ in by 3 in, which help to eliminate disturbances picked up by the input wires. One electrode is placed over the point where the femoral nerve enters the rectus femoris muscle which, according to Tuttle and MacEwen (3), is half-way between the superior margin of the patella and the anterior superior spine of the ilium. The second electrode is placed peripherally about six inches from the first. Control tests have shown that the amplifying-recording unit is stable and able to respond instantaneously to changes in potential impressed on the input.

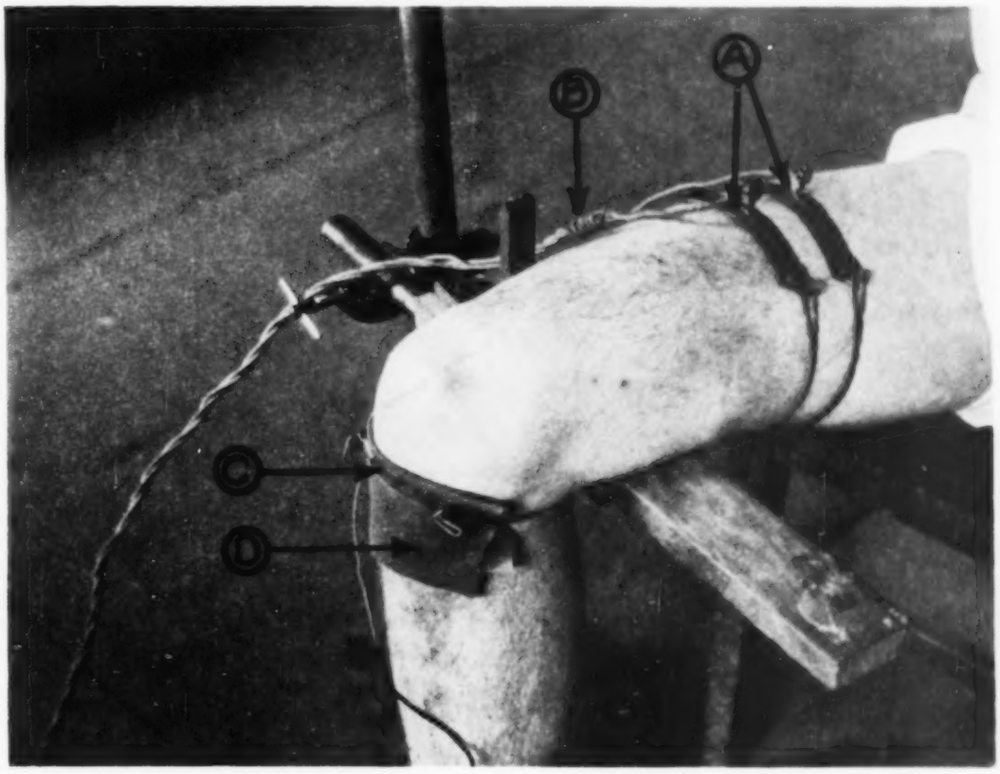


FIG. 6. Photograph showing the placement of the electrodes and metal strip. *A* = electrodes; *B* = input wires (note free end of third wire); *C* = brass plate over patellar tendon; and *D* = rubber dam which insulates the signal circuit from the subject.

The relative positions of the various pieces of apparatus deserve some consideration. We have found it desirable to place the oscillator at a considerable distance (40 feet) from the amplifier. The oscillograph is then placed midway between these two units. The subject should be close to the amplifier in order to reduce to the minimum the length of the input wires. A twisted three-wire conductor served as the input to the amplifier. Two of these wires connected the electrodes with the input of the amplifier while the third was left open at the electrode end and grounded at the amplifier end.

The subjects were comfortably seated with the thigh slightly elevated to put some tension on the quadriceps muscles. An explanation of the manner in which the experiment was to be carried out was given each subject in order to dispel a possible fear of being shocked or injured. Care was taken also to select individuals who were not fatigued and who had not been recently ill. According to unpublished studies the rate and intensity of stimulation of the patellar tendon need not be kept constant because variations in these two factors do not produce a detectable difference in reflex time.

Reflex time is determined in a record by ascertaining the length of time elapsing between the instant of application of the stimulus to the patellar tendon and the arrival of the action currents at the first electrode. Reflex times are read and reported in thousandths of a second. At least eight records were obtained from each subject.

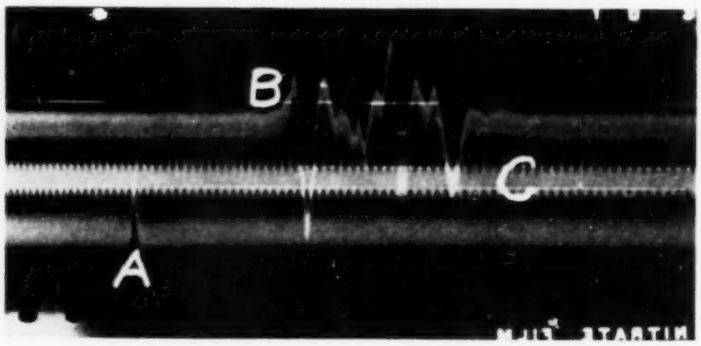


FIG. 7. A typical record of the patellar tendon reflex time. Reading from above downward, the first record is of action currents, the second of the time line and the third of the signal line. The break in the signal line at the point *A* indicates the instant of stimulation of the patellar tendon. The time of arrival of the action currents is shown at point *B* on the action current record. *C* = the time record which indicates time in thousandths of a second. To determine the reflex time or the length of time for the nerve impulse to be set up and to travel from point of stimulation (patellar tendon) to the executant muscles (quadriceps femoris) count the thousandths of second intervals between *A* and *B*.

All experiments on reflex time were made and the records read in ignorance of the results from the intelligence tests. Fifteen records were selected at random and read by a disinterested person. The agreement between the two readings was practically perfect.

INTELLIGENCE TESTS

For estimates of intelligence with which to correlate reflex time we used the Otis test of mental ability (higher exami-

nation, Form A) and the University of Iowa qualifying examination (1927). The former test has a reliability coefficient of .92 and the latter .95. The 30-minute time-limit was used with the Otis test. The University of Iowa qualifying examination required $3\frac{1}{2}$ hours and included the Iowa high school content examination, the Iowa comprehension test, the Iowa placement examination (English training) and the Iowa placement examination (mathematics aptitude).

The raw scores of the four tests which make up the Qualifying examination were weighted approximately inversely as the standard deviations of their distributions in order to give a composite score. The Iowa high school content scores were multiplied by 1, the Iowa comprehension scores by 4, the Mathematics aptitude scores by 3, and the English training scores by 1.[4]

We used two groups of subjects. One group of 44 men and women included graduate and undergraduate students, store and bank clerks, stenographers, housemaids and farm hands. These individuals ranged from 17 to 32 years of age. The second group consisted of 43 University of Iowa freshmen (boys) ranging in age from 16 to 21 years. The first group took the Otis test. The freshmen had taken the Qualifying examination earlier in the fall and were selected from a list arranged in the order of percentile rank to give a fairly normal distribution of scores.

Results

Table I shows that reflex time correlates .87 with the scores of the Otis Test and the composite scores of the University qualifying examination. The correlations between reflex time and the scores of the tests which compose the Qualifying examination are also relatively high. All probable errors are small. The individual with a high score furnished a short reflex time, while the individual with a low score furnished a long reflex time. The correspondence between the intelligence ratings and the reflex times was close, not only for widely separated individuals but for the intermediate subjects as well.

The mean score on the Otis of the forty-four subjects was 53.9 as compared to 53.0 obtained on 2,516 college students reported by Dr. Otis. The range of our group was from 13 to 75 as compared with the range of 20 to 75 in his group.

[4] Personal communication from George D. Stoddard, University of Iowa.

TABLE I

RELATION BETWEEN REFLEX TIMES AND INTELLIGENCE TEST SCORES AS SHOWN BY THE PRODUCT-MOMENT METHOD

	Otis Group (N = 44) Reflex Time	Univ. Freshmen (N = 43) Reflex Time
Otis	.87 ± .024	
Univ. Qual. Exam. (composite score)		.87 ± .025
Iowa Placement; English training		.81 ± .020
Iowa High School Content		.79 ± .022
Iowa Comprehension		.77 ± .025
Iowa Placement; Math. aptitude		.71 ± .030

The mean score of the forty-three freshmen on the University qualifying examination was 362.0, which is approximately the mean of all freshmen students. The range of our group was the same as that of the entire freshman class inasmuch as every tenth student (which included the highest and the lowest) was selected from a list arranged in the order of percentile rank. Each group gave a fairly normal distribution of test scores.

The mean reflex time of the forty-four Otis cases was .0194 sec, with a range from .0114 to .0268 sec. The mean reflex time of the forty-three university freshmen was .0197 sec, with a range from .0154 to .0245 sec. Each group gave a fairly normal distribution in reflex time. Reflex time is relatively constant for the same individual under normal conditions and seemingly independent of the height or size of the subject. It is of interest to note that an individual slightly over six feet tall furnished the shortest reflex time (.0114 sec).

DISCUSSION

The great individual differences in reflex time are considered as very significant. According to the range in the Otis group (0.0114 to 0.0268 sec), conduction over a reflex arc in one individual may be two-and-a-half times as fast as conduction over the same reflex arc in another individual. This marked difference in conduction rate may be due to differences in the conductance of the whole arc or to differences in conductance of only a part or parts of it. It is quite common to refer such a large difference to differences

in synaptic resistance; but we see no reason why a part of the difference at least may not be due to dissimilarities in the receptor, the nerve trunk or the motor end-plate. The work of Erlanger and Gasser (4) on the conduction rate of the nerve impulse in fibres of the sciatic nerve of the frog indicates that such a difference may be referable to the nerve trunk. They found that the nerve impulse travels at different rates in different fibres of the same nerve.

The individual differences in rate of conduction suggest their real significance when viewed in connection with the complicated associational paths of the cerebral cortex which probably function in the higher mental processes.

Just how much of the nervous system is involved in the patellar tendon reflex is a debated question; but a recent study by the writers (5) showed that the records of the action current during the knee-jerk were practically identical with those during voluntary extension of the foot. Both sets of records gave indication of two different rates of discharge, one furnishing the audio or principal frequency of from 300 to 600 oscillations per second, and the other the modulating inaudible frequency of from 8 to 12 per second. This slower rate of discharge is probably from the Betz cells in the precentral cortex (6) and would indicate that the higher centers are involved in the knee-jerk.

Studies which we are now carrying on relative to the effects of mental and physical diseases, stuttering, emotion and drugs upon reflex time are showing still more clearly that we are dealing with a considerably larger fraction of the nervous system than is commonly conceded in this connection. At any rate, we believe that we have in the type of conduction through the patellar reflex arc a sample of the type of conduction that takes place in the higher and probably in the highest nerve centers.

A question that is asked either directly or indirectly is 'What difference does it make whether the nerve impulse travels slowly or rapidly so long as it passes through the arc or arcs involved?' Our answer is that the more of the nervous system (the higher centers especially) that is active within

a given interval of time the more apt is there to be an adequate adjustment or response. This means that speed of transmission of nervous energy from one reaction pattern to another or even through different parts of the same reaction pattern is of prime importance. Such a concept harmonizes with the physiological law of conditioned response; namely, the approximate simultaneity of activity of two action-patterns to be associated.

Intelligence is probably best defined as the ability to see relationships and meanings by having access to as many alternatives or judgments as possible at approximately the same instant of time. This would necessitate the reaction patterns which subserve the judgments to be active within an extremely short interval of time. The 'feeble-minded' individual has, relatively speaking, such a slow conduction rate that one reaction pattern becomes inactive by the time another becomes active, thus doing away with the very factor, relative simultaneity of activity, which makes possible the seeing of a relationship between ideational elements. Prompt radiation of the nerve impulse into a large number of associational systems is probably the neuro-physiological basis of an intelligent response to a complex situation. The cognitive processes may be thought of as hierarchies of reflexes which are compounded into integrated action systems. Conduction through these arcs is undoubtedly very similar to conduction through the arc or arcs which we have been studying and these higher arcs must present marked differences in conductance from individual to individual. The central arcs thus may have a 'personal equation' just as individuals taken as a whole have.

The outcome of this study supports Spearman's theory of intelligence (7) by presenting a neuro-physiological envisagement of the g factor. The general rate of conduction in the nervous system would afford the basis for "general ability" or the "common factor." Just when in the growth and development of the individual a stabilized rate of transmission of the nerve impulse appears is problematical. Is the individual born with a rate which remains constant through-

out life or does the rate change as physiological maturation proceeds?

It is entirely possible that at certain age-levels there would be a very low or even a negative correlation between test scores and patellar tendon reflex time. This is conceivable if we think of the patellar reflex arc as including higher centers which may mature at various ages in various subjects, thus affording unlike lengths of neural tissue for the nerve impulse to traverse. That the knee-jerk may appear when certain parts of the nervous system which ordinarily influence it are eliminated is shown in such pathological conditions as spastic paralysis. Here the upper motor neurone is destroyed and the knee-jerk not only appears but is exaggerated. Such facts would indicate that this reflex may be served by arcs of different length in the same individual, a state of affairs conceivably possible in the maturing child and a situation which would afford the basis for differences in the reflex time in the same child over a period of years. Thus at a certain chronological age we might find a low or negative correlation between intelligence and reflex time because of great differences in the physiological development which the children may present. So far all we can say is that for adults there is a high positive correlation between whatever the intelligence tests used measure and reflex time.

Summary

1. A group of 44 adult subjects (men and women) gave a correlation of .87 ± .024 between scores on the Otis higher examination (Form A) and reflex time.

2. A group of 43 university freshmen (boys) distributed evenly over the range gave a correlation of .87 ± .025 between University of Iowa qualifying examination scores and reflex time. Reflex time correlated with the Iowa placement (English training) scores .81; with Iowa high school content examination it scores .79; with Iowa comprehension test .77; and with Iowa placement (mathematics aptitude) .71.

3. Each group gave a fairly normal distribution of intelligence scores and of reflex times.

4. Wide individual differences in reflex time were found. The range was from 0.0114 to 0.0268 sec.

5. The rate of transmission of the nerve impulse was thought to be important in intelligent responses because the more of the nervous system (especially in the higher centers) that is active within a given interval of time the more likely is there to be an adequate response to a complex situation.

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CONDITIONS AFFECTING HUMAN PAROTID SECRETION

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The publication of Pavlov's *Conditioned Reflexes* has resulted in renewed interest in the careful technique and the significant analyses of this great Russian physiologist. Pavlov's concepts of inhibition, induction, irradiation and concentration of nervous processes in the cerebral cortex, supported as they seem to be by an array of facts, afford a challenge to all students of animal behavior. Pavlov, however, warns against the current tendency of some psychologists to assume that these principles apply also in human behavior.[1] It would seem, then, that a logical program for students of human behavior would suggest the application of the same technique to man, so far as is possible, and the avoidance of any questionable inferences from experiments with dogs. Fortunately this has been made possible by the development of the suction disc cup for draining the salivary duct. As long ago as 1916 K. S. Lashley designed and described this instrument, which is both practical and effective. At the time he presented his device he reported his investigation of the most influential stimuli which appear to affect the secretion of the parotid gland either directly or indirectly and reviewed the literature on the subject.

[1] He opens his last chapter with the following sentences. "In applying to man the results of investigation of the function of the heart, digestive tract and other organs in the higher animals, allied as these organs are to the human in structure, great reserve must be exercised and the validity of comparisons must be verified at every step. Obviously even greater caution must be used in attempting similarly to apply our recently acquired knowledge concerning the higher nervous activity in the dog—the more so, since the incomparably greater development of the cerebral cortex in man is preëminently that factor which has raised man to his dominant position in the animal world. It would be the height of presumption to regard these first steps in elucidating the physiology of the cortex as solving the intricate problems of the higher psychic activities in man, when in fact at the present stage of our work no detailed application of its results to man is yet permissible."

Because of the significance of his work the full report of his conclusions follows.

Direct reflexes of the parotid gland are excited by mechanical, chemical and protopathic stimulation of the oral mucosa.

There is probably no direct reflex to thermal stimuli unless these are of a protopathic intensity.

The secretion produced when a foreign object is chewed involves a specific reaction to a complex group of stimuli.

The reaction of the parotid is most intense when the homolateral mucosa is stimulated.

The quantity of secretion shifts with various chemical stimuli when applied to the various parts of the mucosa; but it is usually greatest when the proximal of the tongue is stimulated.

The presence of food in the stomach excites secretion.

There is no direct reflex to olfactory or to ordinary visual, auditory or tactile stimulation.

Parotid secretion is partially inhibited by violent muscular activity. Mental work (involving movements of the tongue and throat) increases the secretion.

Erotic emotion reduces the quantity of secretion.

Reflex secretion is excited by the sight and odor of food; but is conditioned by hunger, by the previous experience of getting food under the conditions of the experiment, and by complex emotional factors which have not been analyzed.

Ten years after this work was done at Johns Hopkins, N. Krasnogorski, who had been working for many years on motor conditioned reflexes in children, also developed an instrument for collecting the salivary secretion.

Although he worked without knowledge of Lashley's invention, there is only a very slight difference between the two devices. Krasnogorski added a silver sieve for the protection of the mucosa and provided an additional chamber to localize and control chemical stimulation. His work was done in the Child Clinic of the Medical Institute of Leningrad where he was able to use the recording devices of Pavlov. Krasnogorski's work consisted in an investigation of the effect of mechanical and chemical stimulation in various parts of the oral cavity and of a comparative study of the secretion of the parotid and submaxillary glands under various situations. His results are essentially the same as those of Lashley in so far as they are comparable. It should be added, however, that it appears from K's reports that he has been successful in conditioning the salivary glands in the human being.

The purpose in this paper is to report a further investigation of the conditions affecting the secretion of the parotid in man. Some preliminary observations seemed to suggest that influences not yet reported needed to be taken into consideration before an adequate control of secretion could be assured. Since making the study it has seemed worth while to report also any evidence found that supports Lashley's work because of the controversial nature of preceding studies and the scarcity of reliable data on this subject.

Suction disc cups patterned after Lashley's design were employed for the experiment. Since changes in the viscosity of the saliva under various conditions made a difference in the size of the drops, counting the drops was discontinued and the flow of the secretion was measured in cubic centimeters. The same male adult was used as subject for all the trials reported in order that the relationship of the various stimulating factors might be shown.

Whenever areas inside the oral cavity were being stimulated the cups were attached to both Stensen ducts so that the effect of direct stimulation could be observed. When, on the other hand, the stimulation was indirect, a record of one duct proved to be reliable and satisfactory. An examination of the tables which follow will show that when the stimulation is indirect there is no significant difference in the flow of the two glands, but rather a phenomenal agreement in their functioning.

TABLE I

THE SENSITIVITY OF THE PAROTID GLAND TO VARIOUS SITUATIONS

Feb. 27 9:45 P.M. *Stimulation During Successive 3-Minute Periods* Situation	Secretions (cc) Left Stenson	Right Stenson
1. Seated waiting for experiment to begin	.4	.4
2. Seated; no 'muscular activity'	.3	.3
3. Swallowing movements each five sec	.6	.6
4. Same as 2	.3	.3
5. Swallowing movements each five sec	.7	.7
6. Same as 2	.3	.3
7. Mouth wide open (yawned five times)	1.8	1.8
8. Same as 2	.4	.4
9. Same as 7 (yawned five times)	1.7	1.8
10. Same as 2	.4	.4
11. Hopping; 30 sec on rt foot, then 30 sec on left	.4	.4
12. Seated; breathing rapidly as a result of strenuous exercise (perspiring freely)	.8	.9
13. Still perspiring; cooling somewhat	.6	.8
14. Getting cooled off	.4	.6
15. Lying on a couch	.2	.2
16. Sneezed four times	1.5	1.5

Table I is designed to show the sensitivity of these glands to muscular activity involved in such processes as swallowing, yawning, sneezing, and in such gross bodily activity as hopping. The pronounced increase in glandular activity concurrent with the muscular activity involved in furthering both ingestion and digestion is perhaps an unconditioned reaction which nature has provided for effective mastication. The glands secrete equally and simultaneously under this type of stimulation.

In the hopping activity shown in Trial 11 of this table, a more involved source of stimulation appears. In spite of the strenuous activity of the muscles of the body no increase in the flow of saliva appeared during the period of exercise. The amount doubled, however, in the period that followed, although the subject was then sitting. It would seem that an increased flow of blood, a change in body temperature, or more

rapid breathing might be the cause of the increase. A check was made to ascertain which one of these changes was most potent in the situation. The subject took one deep inhalation per second without increasing the flow. When he reclined in a tub of warm water, however, and the temperature of the water was gradually raised until the subject was freely perspiring, the flow of saliva increased 100 per cent. and then gradually decreased as the temperature of the water was reduced. More extended research must be made before one can be sure of the source of stimulation in this case; but the evidence points to a relationship of the factors stimulating perspiration and the factors stimulating the parotid.

Trial 15 of the same Table shows how the glandular secretion slows down when the subject assumes a relaxed posture. By lying quietly with muscles relaxed, the secretion was reduced to a minimum.

TABLE II

EFFECT OF VOCALIZATION UPON PAROTID SECRETION

Feb. 29 7:46 P.M. *Stimulation During Successive 3-Minute Periods* Situation	Secretion (cc) Left Stenson	Right Stenson
1. Reading silently (seated at desk. Vocalization avoided in silent reading)	.3	.3
2. Reading aloud (seated at desk)	.6	.6
3. Reading silently (seated at desk)	.3	.3
4. Eyes closed (seated at desk)	.2	.2
5. Reading aloud	.5+	.6
6. Reading silently (lying on a couch)	.2	.2
7. Reading aloud (lying on a couch)	.4	.4
8. Reading silently (lying on a couch)	.2	.2
9. Counting aloud (seated at desk)	.5	.5
10. Singing (seated at desk)	.5	.5
11. 3-place multiplying	.2	.2
12. 3-place multiplying	.2	.2
9 minutes silent reading	10	10
9 minutes oral reading	15	16

Table II shows evidence of direct stimulation from the muscles involved in vocalization. Whether seated or reclining on a couch the secretion was materially increased when the subject read aloud, counted or sang. Unless a special effort was made to avoid vocalizing in silent reading, the response was more nearly equal to that of oral reading. When the same song was pitched one octave higher, so that the muscles were strained, there was a decrease in the secretion. Several attempts were made to determine whether or not problem-solving had an inhibitory effect upon the processes of secretion. The results seemed to vary as more or less vocalization was involved in the process.

In Trials 11 and 12 of this Table this result is shown. Three-place numbers were multiplied without visual aid and a diminution of secretion is shown; but whether these results were due to lack of stimulation or to inhibition is a problem for further investigation. This type of investigation might be used to determine the extent to which thinking involves laryngeal movements.

Table III shows the difference in the activity of the two glands when the food is chewed on one side of the mouth. In conditioning the parotid gland it would seem to be very important that the unconditioned response should be based

TABLE III

EFFECT OF CHEWING ON ONE SIDE OF THE MOUTH

Feb. 25 1:45 P.M. *Stimulation During Successive 3-Minute Periods* Situation	Secretion (cc) Left Stenson	Right Stenson
1. Chewing a small piece of rubber between the molars of the left side. No swallowing	2.00	.4
2. Chewing the same rubber between the right molars. Chewing faster than before	1.00	3.6
3. Same as 1	2.2	.8
4. Chewing pieces of apple on right side	2.3	8.9
5. Chewing pieces of apple on left side	9.1	2.3
6. Chewing a graham cracker on right side	.8	2.5
7. Chewing a graham cracker on left side	2.7	1.0
Total amount on the side of greatest stimulation	31.00	
Total amount on the opposite side	8.6	

upon something other than chewing, since a shift of food in the mouth would be a disturbing factor in discriminative research. A drop or two of acid in a definitely determined location within the mouth would be a more constant type of stimulation.

The significance of the condition of the stomach as a disturbing organ is shown in Table IV. The successive five-minute periods preceding lunch regularly yielded about 4 cc. Lunch was then eaten and all particles of food were removed from the mouth and the same posture resumed. Successive periods then yielded a 100-per cent. increase over the preceding periods. Whether the walls of the stomach or chemical activity within the digestive tract is responsible for this increase might be decided by artificially inflating the stomach walls. It should be incidentally noted that there was no increase in the secretion at the usual time for luncheon.

Table IV

Effect of food in the stomach on parotid secretion

Feb. 17	*Sources of Stimulation During Successive 5-Minute Periods* Situation	Secretion from Right Parotid (cc)
12 M.	1. Seated; reading silently	.4
	2. Same	.4
	3. "	.4
	4. "	.4
	5. "	.5
	6. "	.4
	7. "	.4
	(These experiments were made during the subject's usual luncheon hour.)	
12:40 P.M.	Subject lunched, after which particles of food were removed from the mouth and the disc cup was replaced.	
1:20 P.M.	8. Seated; reading silently	.8
	9. Same	.9
	10. "	.9
	11. "	.8
	12. "	.7

Table V

Effect of strained muscular activity on the secretion

Feb. 25	*Stimulation During Successive 3-Minute Periods* Situation	Secretion (cc) Left Stenson	Right Stenson
3:27 P.M.	1. Subject standing relatively still	.4	.4
Lunch at 1 P.M.	2. Lying, supporting body on hands and toes; raising body up and down with 50 lbs on back as often as strength would permit	.3	.25
	3. Resting; breathing heavily, perspiring	.5	.5
	4. Same as 2	.3	.3
	5. Resting; breathing heavily, perspiring	.5	.6
	6. Same as 2	.3	.3
	7. Resting; cooling off	.6	.5
	8. Resting; cooling off	.2	.2
	9. Cooling off	.4	.4
	Secretion during violent exercise (9 min)	1.75	
	Secretion immediately following (9 min)	3.20	

The data shown in Table V afford an example of what might be interpreted as inhibition of the centers controlling the parotid gland.

In the second trial this subject stretched out on the floor in a horizontal position supporting the body on his hands and toes. A fifty-pound weight was placed on his back and he proceeded to lower and raise his body without resting it on the floor. This exercise involved a violent straining of the musculature. At the end of the period the subject was exhausted. In spite of the increased vascular action and

excessive perspiration (which are probably excitatory factors), there was a definite decrease in the secretion during the period of straining. The improbability of any other explanation except inhibition is apparent in the light of the reaction in Trial 12 of Table I. Inhibition was obtained, however, only when the strain was painfully severe.

Certain emotional states aroused during the research also gave evidence of inhibition; but this very important aspect of the subject is being studied as a special problem and will be reported later.

In the light of Pavlov's theory that sleep is an inhibition, an attempt was made to get a record of the activity of the glands during sleep. As has been previously noted, the secretion was reduced when the subject assumed a reclining posture. Upon three different occasions the subject was able to go to sleep with the disc cups attached. The length of the periods and the amount of secretion are shown in Table VI. There was an interval between the taking of the last

TABLE VI

EFFECT OF SLEEP UPON PAROTID SECRETION

	Situation	Secretion (cc) Left Stenson	Right Stenson
Feb. 13 3:30 P.M.	1. Lying on a couch dozing intermittently for 15 minutes	—	.2
Feb. 15 10:15 P.M. to 11:30 P.M.	2. Asleep for 75 minutes	—	.2
Feb. 29 11:15 P.M. to 12:45 A.M.	3. Asleep for 90 minutes	.4	.4

measure and the onset of sleep which might easily account for the small amount of secretion shown. It appears that there is very little, if any, secretion during sleep. But whether the reduced amount is due to inhibition or to a lack of stimulation is an unsolved problem.

Since these experiments were undertaken as a preliminary study for conditioning the parotid gland, the experimenter was interested in any evidence of conditioning that might be noted. On many occasions the secretion was increased when

the subject sat looking at food, particularly if it was a situation in which he had formerly partaken of the food visualized. The peeling of an orange or the squeezing of juice from an orange was always effective in causing a marked increase in the salivary secretion (Table VII). This increase was not comparable in volume to the amount evoked by direct stimulation but it was usually from 100 to 200 per cent. more than the flow without the visual stimulation. As soon as the stimulating agent was removed from view the secretion dropped back to normal. When the stimulation was prolonged without re-enforcing the unconditioned response by tasting the orange juice, the flow soon fell down to the level of the non-stimulated period. In the human subject, this unconditioning process would seem to proceed much more rapidly than was the case with Pavlov's dogs.

TABLE VII

EVIDENCES OF CONDITIONED REACTIONS

	Successive 3-Minute Periods Situation	Secretion (cc) Right Stenson	Left Stenson
Mar. 3	1. Subject seated (no stimulation)	.2	.2
9:02 P.M.	2. Squeezing the juice from an orange	.6	.5
	3. Orange removed from view	.3	.3
	4. Eyes closed, smelling orange	.4	.4
Mar. 7	5. No stimulation	.1	.1
8:30 P.M.	6. Looking at a slice of raw onion through glass (no smell apprehended)	.3	.3
	7. Subject blindfolded; onion held near nose	.1	.1
9:00 P.M.	8. Subject blindfolded; peppermint candy held near the nose	.1	.1
	9. Looking at the peppermint candy	.4	.4
	10. Drank two spoonfuls of orange juice	6.4	6.4
	11. No stimulation	1.4	1.6
	12. No stimulation	1	1

An attempt was made at several sittings to see which sensory factor seemed to be most potent in the conditioned response. It will be noted in Table VII (trial 6) that when the onion was shown through a glass designed to shut out olfactory stimulation the response was still pronounced. On the other hand, whenever olfaction alone was used (as in

Trial 8), there was no evidence of a conditioned response; but as soon as the blindfold was removed and the visual area stimulated a definite conditioned response was shown. This would seem to suggest that in this particular subject the conditioned reaction to the stimuli used was very largely visual. There are undoubtedly great individual variations depending upon experiential factors entering here which deserve careful and extensive investigation.

On the whole, this preliminary survey of the conditions affecting human parotid secretion suggests the need for further work in constructive conditioning. Theory and inference have advanced far beyond the warrant of the meagre amount of available experimental evidence. The study of salivary secretion affords an inviting approach to such important aspects of behavior as association, differentiation and inhibition. The variety and complexity of the stimuli involved, the freedom from 'voluntary control' and the definiteness of the response meet the most exacting requirement for a scientific study of human behavior.

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THE RELATION OF REACTION TIME TO PHASE OF BREATHING

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Various aspects of breathing have been widely studied in the field of emotion. The conventional experiments by the expressive methods were concerned with the rate and the depth of breathing as correlated with feeling and emotion. More recently Benussi [1] and others have been exploiting the possibilities of the time-ratio between inspiration and expiration, with special reference to detecting the emotions present during false report. Collaborating with Burtt and Troland, Marston has made a study of the same ratio and of other symptoms of deception with a view to military use.[2] Again, investigations of emotional stability as indicated by breathing under a sudden strong stimulus have been carried on for the vocational selection of aviators and taxi drivers,[3] and Larson has used disturbed breathing in detecting deception.[4]

In a study of the inspiration-expiration ratio by Burtt [5] there was a hint that breathing was in some way related also to attention. Burtt thought it possible that concentration upon true replies to questioning might increase the I/E ratio, with a subsequent return to normal. Change in breathing when concentrating on a problem is a common experience. We hold the breath when making a difficult sensory discrimination, and on the rifle range the customary procedure is to take a deep breath, exhale slightly and then hold the

[1] V. Benussi, Die Atmungsymptome der Luge, *Arch. f. d. ges. Psychol.*, 1914, 31, 244–273.

[2] *Cf.* J. F. Shepard, Review of affective phenomena, *Psychol. Bull.*, 1919, 16, 244.

[3] *Cf.* V. A. C. Henmon, Air Service tests of aptitude for flying, *J. Appl. Psychol.*, 1919, 3, 103–109; A. J. Snow, Selecting taxi drivers, *Indust. Psychol.*, 1926, 1, 30–45.

[4] J. A. Larson, A cardio-pneumo-psychogram in deception, this JOURNAL, 1923, 6, 420.

[5] H. E. Burtt, The inspiration-expiration ratio during truth and falsehood, this JOURNAL, 1923, 4, 1–23.

breath while aiming and firing. It appears that the phase of breathing (*i.e.* inhaling, exhaling or holding) may be crucial in such cases. This is our problem.

Reaction time was the process selected as the other variable. It has the advantage of being entirely objective and it is possible to amass readings in a reasonable time, thus increasing the statistical significance of the results. A stimulus was presented at the beginning of inspiration, during inspiration, at the beginning of expiration or during expiration. Simple auditory and visual reactions were used.

The subject wore a Sumner pneumograph connected with a special tambour which closed an electric circuit during expiration.[6] A complicated circuit involving several relays in addition to that operated by the pneumograph and a rotating disk which closed two contacts made it possible to present the stimulus for inspiration or expiration as desired. Whether it came at the beginning of the phase or during it was a matter of chance; but a signal lamp indicated to the experimenter what had happened. If during inspiration or expiration it occurred at any time during the phase. In successive trials the switches were set for inspiration or expiration in a random order.

A warning was given automatically by a buzzer from 4.0 to 0.7 sec before the stimulus. An electro-magnetic sound-hammer and the disappearance from a grey background of a shadow cast by a vertical rod constituted the stimuli. The subject reacted by releasing a telegraph key suspended from above and grasped between the thumb and first finger.

A Hipp chronoscope (without springs) was used in the auditory series and a Dunlap chronoscope in the visual. Each was calibrated with a fall control three times a week. The average deviation of 125 calibration readings was about 0.1σ. The stimulus control and chronoscope were in a separate room with connections through a small hole in the wall. In the visual series this apparatus was three rooms distant from the subject.

The subjects used were university students. Each experimental period for each subject was constant (50 min) and each subject reported at least ten times, at regular hours and at regular intervals. Thus such conditions as previous class work, daily fatigue and meals were fairly constant. Lighting, ventilation, noise and similar factors were practically constant.

Each subject was cautioned to maintain the same postural attitude throughout. Particular care was exercised in attaching the pneumograph in the same way and in the same relative position on all subjects.

Results. The notations used throughout the discussion are as follows; E = the stimulus at the beginning of expiration, e = the stimulus at random during expiration, I = at the beginning of inspiration, and i = during inspiration.

[6] H. E. Burtt, A pneumograph for inspiration-expiration ratios, *Psychol. Bull.*, 1918, 15, 325–328.

TABLE I

Subject	Medians				Quartile Deviations				*E–I*		*E–e*		*E–i*		*I–e*		*I–i*		*e–i*	
	I	*E*	*i*	*e*	*I*	*E*	*i*	*e*	*D*	*D*/P.E.d	*D*	*D*/P.E.d	*D*	*D*/P.E.d	*D*	*D*/P.E.d	*D*	*D*/P.E.d	*D*	*D*/P.E.d
Auditory																				
A.....	223	229	241	218	44	37	46	40	6	.9	11	2.0	−12	1.7	6	1.0	17	2.4	−23	3.2
B.....	302	286	293	290	69	36	40	29	−16	1.5	− 4	.5	− 7	.8	12	1.0	9	.7	− 3	.3
C.....	260	250	267	267	41	41	45	37	−10	1.1	−17	2.0	−17	2.0	− 7	.9	− 7	.8	0	0
D.....	185	217	230	209	41	39	55	41	32	3.9	8	.9	−12	1.0	−24	2.5	−45	4.2	−21	2.0
E.....	292	283	288	288	45	27	18	24	− 9	.9	− 5	.9	− 5	1.0	4	.4	4	.4	0	0
F.....	210	230	278	223	43	37	44	30	20	2.2	7	.9	−48	2.6	−13	1.3	−68	4.7	−55	4.4
G.....	311	280	306	276	45	29	42	35	−31	3.7	4	.8	−26	2.7	35	3.8	5	.9	−30	3.2
H.....	306	311	324	329	59	56	48	50	5	.7	−18	3.0	−13	1.8	−23	2.7	−18	2.5	5	.7
I.....	255	262	265	257	48	48	48	37	7	1.5	5	.9	− 3	.5	− 2	.4	−10	1.7	− 8	1.3
J.....	246	240	251	242	45	38	30	37	− 6	1.4	− 3	.6	−11	3.0	4	.8	− 5	1.3	− 9	4.5
K.....	183	175	186	170	20	24	21	28	− 8	3.8	5	1.4	−11	4.3	13	4.0	− 3	1.2	−16	4.4
L.....	284	277	282	290	63	46	58	58	− 7	1.1	−13	2.1	− 5	.5	− 6	1.0	2	.2	8	.8
M.....	213	211	214	218	50	36	36	33	− 2	.4	− 7	1.7	− 3	.7	− 5	1.1	− 1	.2	4	.9
N.....	252	239	233	255	46	45	42	45	−13	2.7	−16	3.1	6	1.1	− 3	.6	19	3.6	22	4.0
O.....	217	239	235	218	40	50	47	51	22	4.2	21	3.3	4	.6	− 1	.2	−18	3.0	−17	2.3
P.....	186	174	206	203	36	32	32	35	−12	2.4	−29	5.7	−32	6.6	−17	3.1	−20	4.0	− 3	.5
Q.....	208	250	218	233	43	60	33	39	42	3.6	17	1.5	32	2.7	−25	5.1	−10	1.6	15	2.6
R.....	287	316	300	278	33	16	36	40	29	8.2	29	7.2	16	3.0	0	0	−13	2.2	−13	2.1
Auditory Ave.	245	248	256	248	45	39	40	38												
Visual																				
S.....	242	220	130	80	21	18	17	24	−22	3.2	140	26.0	90	17.3	162	28.0	112	20.0	−50	13.1
T.....	246	234	164	136	14	18	15	19	−12	2.1	198	38.0	70	13.4	110	30.5	82	22.7	−28	9.3
U.....	282	256	190	182	25	24	16	40	−26	3.1	74	7.7	66	9.1	100	11.1	92	14.3	− 8	1.0
V.....	324	312	256	204	32	38	26	23	−12	.9	108	10.0	56	5.0	120	12.7	68	6.8	−52	6.6
W.....	242	246	174	124	16	14	13	20	4	.7	122	24.4	72	15.0	118	23.6	68	14.1	−50	13.8
X.....	240	232	166	120	11	14	11	17	− 8	1.6	112	22.4	66	14.3	120	31.5	74	21.7	−46	14.3
Y.....	208	190	124	88	18	11	10	12	−18	1.5	102	12.4	66	7.8	120	13.6	84	9.7	−36	8.6
Visual Ave....	254	241	172	133	20	20	16	22												
Ave. of Aud. and Vis.....	248	246	232	215	38	33	33	34												

Table I gives the median reaction time for each subject in each phase, the quartile deviations of these same distributions, the differences between these various medians and the ratios of difference to the probable error of difference. The captions at the head of the 'difference' columns should read, for instance, "*E minus I*," in order to give the proper sign to the difference. Averages are likewise given for the medians and quartile deviations.

Considering first the averages of the medians in the auditory series, a difference is present though not striking. The average for the median of *E* is the same as that for *e*. The largest average for the median of *E* is the same as that for *e*. The largest average is that for *i*, and the smallest for *I*. But the difference between the median of *I* and the medians of *E* and *e* is very small. In the averages of the medians for the visual series, more striking differences are shown. By far the smallest average is that for *e*. The average for *I* is quite the largest. In the order of increasing magnitude the averages for the visual series are *e*, *i*, *E* and *I*. If the averages of all the medians, both auditory and visual, for a particular phase are computed, the results again show an appreciable difference. The time is shortest for reactions made during expiration. The longest time is for reactions made at the beginning of inspiration, and the times in order of increasing magnitude are *e*, *i*, *E* and *I*. It seems that, in general, *e* provides the conditions for most rapid reaction while *E* and *I* are the slowest, with *i* taking an intermediate position.

The results are more marked with the visual reaction. No significance is to be attached to the fact that the auditory averages are larger than the visual, contrary to the usual results. The same subjects did not participate in the two series and the auditory group as a whole happened to be slower.

In the right portion of the Table, differences larger than three times the P.E. diff. are considered significant and are indicated in bold face. Of the 150 differences, 66 are significant on this basis; 8 for *E–I*, 12 for *E–e*, 11 for *E–i*, 11 for *I–e*, 12 for *I–i* and 12 for *e–i*.

We are not interested merely in the significant differences but in their direction. If plus and minus values occur with equal frequency in a given column, there is little indication of a fundamental trend. In the comparison of *E* with *I*, four of the eight differences show that *E* is greater (*i.e.*, slower) than *I*, and four the reverse. No general trend. The medians of *E* are larger than those of *e* in nine of the twelve cases.

A significant agreement. Eight of the medians for *E* are greater than those for *i* and three the reverse; nine of those for *I* are greater than those for *e*, and two the opposite; the median for *I* is greater than that for *i* in eight of the twelve significant comparisons; and, finally, there is just one case of *e* greater than *i*, while in eleven cases *i* is greater than *e*.

From this type of analysis it is again evident that the reaction time for *e* is quicker than for any of the other phases. The reactions for *E* and *I* are slower than those of other phases. Those for *i* are of a magnitude intermediate between those of *e* and of *E* and *I*. It is primarily the visual results that contribute to this conclusion, although both series agree in the superiority of *e* to *i*.

We may approach the data from another angle by considering the number of times the *E* median, for example, proves significantly larger than the others and the number of times it proves significantly less. When all the cases of significant differences are considered, the *E* median is greater than the other phase with which it is compared, 47 per cent. of the time in the auditory series, 88 per cent. in the visual. Combining auditory and visual, *I* is greater 68 per cent., less 32 per cent.; *i* is greater 51 per cent., less 49 per cent.; *e* is greater 17 per cent., less 83 per cent.

If we consider only the totals, combining auditory and visual, we come to our former conclusion. Reaction times for *E* and *I* are relatively slow, each of them proving slower than the phase with which it is compared about twice as frequently as it proves faster. On the other hand, the times for *e* are relatively rapid, actually showing this tendency in 83 per cent. of the comparisons. The *i* reactions occupy an intermediate position, being faster than the comparison in about half the cases.

As in the previous analysis, however, the auditory and visual results do not always correspond. The inferiority of *E* is due mainly to the visual series, for the auditory is equivocal. The same is true with reference to the inferiority of *I*. In fact the auditory contradicts the visual since the latter indicates *I*'s inferiority 100 per cent. Both series agree in the superiority of *e*, but disagree again regarding *i*, with the average yielding about 50 per cent.

Individual differences were of course manifest throughout the experiment. Our interest, however, is mainly in the relative speed of a given subject on the four phases. Some subjects obviously show more consistent tendencies than do others.

We have thus far been dealing with central tendencies. It is also of interest to consider the variability of reaction time. The first point to note is the greater variability in the auditory series. This may be seen especially in the averages of the quartile deviations. The writer suspects that this is due to a more erratic group of subjects. Many of the original

protocols, *e.g.*, indicated that these subjects were inattentive during the fore-period.

Another point to consider is the comparative variability of the four phases. If we combine both series, *I* has clearly the greatest variability. Again, we may combine *I* and *E*, on the one hand, and *i* and *e*, on the other. Here we find that *I* and *E* show more scatter in both auditory and visual series than do *i* and *e*. Considerable variation for the various subjects is shown, and the general trends are not so marked as in the case of the medians. There seems, however, to be some indication that the variability is higher on *I* and *E*, where the slower average reaction times were found, and lower on *i* and *e*, where quicker reactions occurred.

Summary. While there are marked individual differences and while the auditory and visual experiments do not always corroborate one another, it seems safe to conclude that the simple reaction time is slower when the stimulus occurs at the *beginning* of an inspiration or expiration than when the stimulus occurs *during* the inspiration or expiration. The most plausible explanation is that the change of phase of the breathing operates as a distraction. Breathing is supposed for all practical purposes to be non-conscious; but it may be that although subliminal it has some effect. It seems plausible that the moment of change of phase should be somewhat unfavorable as regards attention, because at this point there is a resetting of the neuromuscular system. There may be a slight diversion of energy in supplying the breathing muscles or some marginal sensations from the muscular contractions. This would perhaps account for the demonstrated inferiority of *I* and *E*. As to further differences, the case is not clear. We noted that *e* gives a more rapid median than *i*. There is somewhat less effort in exhaling, as a relaxation of the diaphragm is involved, and possibly this minimizes the distraction. Again the greater variability of *I* compared with *E* may be due to the same thing, *i.e.*, to the greater effort of starting an inhalation as compared with that of starting an exhalation. At any rate, our original suspicion that the phase of breathing might exert an influence on reaction time seems to be confirmed.

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EXPERIMENTS ON THE EFFECTS OF THE LOSS OF SLEEP

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Although studies of sleep have been numerous both in psychology and in physiology, the results have chiefly shown the difficulties of the problem and the inaccessibility of the sleeping state to experimental study. The statement of Claparède that "as different as are their conceptions about sleep, all authors on the subject agree on one point—that this problem is surrounded by a thick obscurity which the progress of physiological science has not yet pierced" [1] is as true today as it was twenty years ago. In 1878 Ernest Naville wrote "In Beclard's Physiology (1856) sleep occupies two pages in a total of one thousand and ninety-six; in that of Beaunis one and one-half pages out of one thousand and ninety-eight. Sleep occupies one-third of the time of human life. The disproportion between the place it occupies in life and the place it occupies in books on physiology is impressive." Yet in the more recent texts the situation is not very different, since, although the subject occupies a greater number of pages, the treatment is one of theories, often conflicting, with little that is conclusive.

Some information is at hand about the relation of fatigue to the feelings of fatigue; about the spread of fatigue; about the resiliency of recovery from hard exercise with sound rest as opposed to half-hearted exercise and disturbed rest, and about the effects of fatigue upon the curves of work of isolated muscles and groups of muscles.

A small amount of knowledge has accrued on the desirability of complex tests instead of simple tests for use in experiments on fatigue and loss of sleep; on the optimal duration of the tests used; on the emotions of the subjects toward the experimenter, toward the tests used, toward the experiment, and the instructions for the tests; on the relations of speed to accuracy and of both of these conditions to accomplishment under test conditions. A little may be surmised also concerning the relation of the degree of practice or automatization to the effects of fatigue.

There have been a few outstanding papers on the relation of hypnotic and

[1] E. Claparède, Théorie biologique du sommeil, *Arch. de psychol.*, 1905, 4, 245–349.

hypnoidal states to normal sleep; the relation of the higher levels of the central nervous system to the periphery and to the vasomotor centers preceding and during sleep, and of the spread of inhibitions of conditioned behavior or reflexes to the induction of the sleep state. But little is known of the relation between sleep and fatigue; of the connection between sleep and relaxation or rest; of the physiological or psychological mechanisms producing the sleep state; of the psychological or physiological results of sleep; of the relation of the length to the depth of sleep in catabolic results; of the bases of morbid[2] sleep; of the comparative values of interrupted as opposed to continuous sleep; of sleep at various hours of the night in terms of recuperative values; of the relative values of sleep during the night and during the day, or of hibernal sleep.

Furthermore, but little is known from scientific studies of the relation of desirable amounts of sleep for recuperation from various types and degrees of effort; of the quantities of sleep required (or sought) by the various units of the anatomical structure; of the effects of illness, concomitant distraction, previous distraction, emotional residue, physique, reserve energy, cellular energy, race, sex, light, air, season, food, narcotic drugs, temperature, meteorological conditions, bodily position during rest, health, blows on the cranium, conditions of breathing, and other sensory and physio-psychological factors upon the satisfactoriness of the results of sleep;[3] or of the effects of physiological or psychological shock, of blood content, or of the oxygen supply on the sleep state.

Some authors state that in deep sleep there is absolute repose of the brain; while others state that even in the deepest sleep the brain is partially awake. There are numerous articles on sleep and hysteria, but these are not in perfect agreement. An interesting field of study lies in the sleep habits of 'Siamese twins' and other more or less unusual persons.

There is but little sound knowledge of the most beneficial amounts of sleep in terms of age-norms, physiological age, general metabolic ratios or individual desirability; of the optimal distributions of periods of sleep; of the influence of suggestion, habit, interest, mood, visual or tactual cues or other similar factors upon the approach or the retreat of the sleep state, or of its reconstructive influence if it occurs; of the influence of compensation or compensatory efforts on the results of tests given or work done in industry under loss of sleep conditions. Analyses of compensatory effort or measurement of its temporary or persistent effects under various conditions of mind or body are rare. We know nothing of the genetic development of sleep.[4] We have

[2] C. D. Camp, Morbid sleepiness, with a case of narcolepsy, *J. Abn. Psychol.*, 1907, 2, 9–21; I. H. Coriat, The nature of sleep, *ibid.*, 1912, 7, 329 ff.; B. Sidis, An experimental study of sleep, *ibid.*, 1908, 3, 1–32; M. M. Willey, The psychic utility of sleep, *ibid.*, 1924, 19, 174 ff.

[3] A similar list is given by W. H. Burnham, *Amer. J. Psychol.*, 1904, 19, 385–399.

[4] F. Herz, Selbstbeobachtung ueber freiwillige Schlaf, *Arch. f. d. ges. Physiol.*, 1923, 200, 429–442; H. M. Johnson, An essay toward an adequate explanation of sleep, *Proc. Amer. Psychol. Ass.*, 1925; I. P. Pavlov, On the investigations of the higher nervous functions, *Brit. Med. J.*, 1913 (no. 2755), 973–978; Reize und Schlafe, *Skand. Arch. f. Physiol.*, 1923, 44, 32–58; I. P. Pavlov & L. Wasskressenski, Contribution à la physiologie du sommeil, *C. r. Soc. de biol.*, 1916, 79, 1078–1084; E. F. W. Pflüger, Theorie des Schlafes, *Arch. f. d. ges. Physiol.*, 1875, 10, 468 ff.; H. Piéron, Le problème physiologique du sommeil, 1913; J. F. Shepard, The circulation and sleep, *Univ. of Mich. Stud.*, 1913, 1, 1–83.

even seen proposed the theory that sleep is not a necessity if members of several generations will undergo a sufficient period of training in abstinence from it. There are innumerable advocates of shorter periods of sleep than are customary.[5]

The present study was undertaken in an effort to throw a little light upon a few of the factors related to the sleep state. The complexity of this state and the reactions which precede it have apparently been greatly underestimated by many of the experimenters of the past.

Previous Studies of Fatigue and Sleep

Although the relation of the necessity of, or the desire for, sleep to the degree and the extent of fatigue is not well understood at the present time, there is little question of the close association of the two. Consideration of the one without the other leaves the subject incomplete.

A general weakness of the experiments on fatigue and sleep is the striking persistence of experimenters in using simple physiological criteria for observations on both mental and physical fatigue, and the same assiduous determination in the use of short, simple motor and sensory tests as means of attempting to gauge mental fatigue. While it is probably true that there is some spread of fatigue, at least in the sphere of influence of volition, it can not be assumed that the relationship between the fatigue of one type of muscular effort and that of another type is constant. Still less constant is the relationship between muscular fatigue and mental effort.

The objection to the short test is that the subject is able to exhibit a great deal of energy and to work accurately for brief periods of time if these are intermittent and if he knows that the bursts of effort are to be brief. Under conditions of fatigue, however, he can not sustain these for more than a few minutes of time. This compensatory effort is limited in its liability to call. The objection to simple tests is that they become rapidly habitual or automatic and that they can then be performed with a minimum of effort and attention. In fact, they may become so automatic that attention to them interferes with their performance. The part played by experience may become so small as to seem (subjectively) to be unessential.

Numerous theories of the causes of sleep have been offered, but none has had general acceptance. Chronologically, the first of the modern theories[6] was that of cerebral congestion of blood which so choked the nerves that the passage of nervous fluid or the oscillation of cerebral vibration was interrupted. A second theory is that of cerebral anæmia as the cause of sleep. There is adequate evidence to show the presence of cerebral anæmia during sleep; but numerous experiments demonstrate that this poverty of blood in the cerebrum as the cause of sleep remains to be scientifically proved.

[5] R. MacNish, *The philosophy of sleep*, 1830.

[6] Extensive reviews of the theories of sleep have been made by W. H. Howell, A contribution to the physiology of sleep, *J. Exper. Med.*, 1897, 2, 314–345; H. H. Foster, A new standpoint in sleep theories, *Amer. J. Psychol.*, 1900, 12, 145–174; Ed. Claparède, Théorie biologique du sommeil, *Arch. de psychol.*, 1905, 4, 245–349.

Other theories based on the circulation of the blood or the oxygen-carrying power of the blood have been offered from time to time but have not been received with general credence. Theories based on the morphological or physiological reactions of the nervous system as the cause of sleep have tended to consider (1) the withdrawal of the dendrites from contact at the synapses, but this movement of the dendrites has not been demonstrated; (2) interposition of pseudopodia from the neuroglia cells between the points of contact at the synapses, but this, too, has yet to be demonstrated; (3) changes of cell size and structure that seem to be coincidental with sleep have been offered as having a causal effect in relation to sleep, but serious doubts of the technique of the experiments on which the theories were based have yet to be answered; and (4) interference between the sympathetic and the central nervous systems has been offered as a theory but without general acceptance.

Inhibition of reaction has received a certain amount of attention as the cause of sleep; but the theories of this type rarely offer an origin or theory of the causative mechanism of the inhibition, *e.g.*, the inhibition resulting from a combination of visual and kinæsthetic cues based largely on the appreciation of surroundings customary to the approach of sleep, or inhibition resulting from overstimulation of some area of the central nervous system.

The theories of chemical causation of sleep may be divided into two classes, the bio-chemical and the toxic. The bio-chemical involve the quantity of oxygen in the blood or its distribution in the body. The toxic theories involve the inadequate elimination from the body of the products of activity.

The theory that sleep is an instinct—an active, positive condition and not a negative state of cessation of activity—has been presented, but has not attracted the general interest which it seems to deserve.

Psychological experiments on the loss of sleep. The list of published psychological experiments on the effects of the loss of sleep does not include many names nor does it cover a long period of time. The earliest observations of an experimental nature known to the writer were the effects on association of the loss of small amounts of sleep by Aschaffenburg [7] and by Roemer.[8]

Gilbert and Patrick [9] used a large number of simple tests on three subjects who remained awake for a period of ninety hours. Their experiments were conducted in two parts, one on a single subject and the other on two subjects. The tests used were simple reaction times, choice reaction times, tapping, memory for nonsense syllables, addition, dynamometric pressure, dynamometric lift, sensibility to pain (algometer) and acuteness of vision. In the second part of the experiment 172 digits in groups of nine were memorized in place of the nonsense syllables; letter-naming was substituted for reading; and a test of acuteness of hearing by discrimination of intensity was added. Observations of the physical effects of this experiment were made on the pulse

[7] G. Aschaffenburg, Experimentelle Studien ueber Associationen, *Psychol. Arb.*, 1895, 1; 1897, 2.

[8] E. Roemer, Tätigkeiten, *III. Intern. Kongr. f. Psychol.*, 1896.

[9] J. A. Gilbert & G. T. W. Patrick, On the effects of the loss of sleep, *Iowa Univ. Stud. Psychol.*, 1897, 3, 469 ff.

before and after each series of tests, and on the bodily temperature, bodily weight and composition of urine. Practice periods on some of the tests were held for two or three days preceding the experiment. The records for the last day preceding the loss of sleep were taken as normal standards. Tests were held every six hours during the experiment. One subject had a large amount of hallucinatory experience. The others had none at all. Vision improved throughout the period of the experiment, and was poorer in the first test after the sleep that followed the close of the experiment. The quality of vision was not parallel to the discomfort felt in the eyes. There was overcompensation in some of the tests immediately following sleep. One of the subjects, apparently awake, dreamed while standing and lifting small pails as a test of weight discrimination. A study of the results gives a hint of cycles of sleepiness in the subjects as they did better in most of the later tests than at any other time during the period of insomnia, although some effects of practice probably remained.

A second psychological experiment was that of Miss May Smith, 1915.[10] Miss Smith reduced her sleep to one and one-half hours the first night, to three and one-half the second night, and to five and one-half the third night. She acted as her own subject, using as tests the Rivers-McDougall dotting machine, memory for connected words, reversible perspective (windmill), tapping, and learning and relearning nonsense syllables. She carried on these tests for a long period preceding and succeeding the period of diminished sleep, making her experiment the most complete on the qualitative side that has been made. As a rule she made a better score on the second day of diminished sleep than on the first; her recovery was so slow that she did not consider it complete for two weeks or longer; a temporary return to normal performance was achieved by a repetition of the diminished sleep (as conducted before), but the ultimate return to normal was delayed greatly by such a repetition. Miss Smith places great emphasis on the first and second cycles of the fatigue state. It is quite possible that these are shorter than she believed and are recurrent in a manner similar to the cycles of the seizures of hunger. The length of Miss Smith's practice period may have reduced her performance on some of the tests to almost pure automaticity, even though her interest in the tests was not lost.

Hermann and Robinson tested three subjects on a number of simple performances once each day over a practice-period of eleven to twenty-six days and over a period of loss of sleep of sixty hours.[11] The tests used were the hand dynamometer, aiming, naming letters and mental multiplication of a series of twelve pairs of two-place numbers. The authors found no objective effects from this period of loss of sleep, possibly because their tests were too short and too simple, as they themselves suggest.

Robinson and Robinson used a group of twenty-five college students as an insomnia group over a twenty-four hour period of loss of sleep, checking the results obtained against those from a similar group of thirty-nine students who slept according to their usual habits.[12] The tests employed were various forms of the Army Alpha. Three forms were given; one preceding, one at the close of the sleepless period and one after the period of sleep that followed the testing period. There were no noticeable effects except those from practice and those from the greater earnestness of the insomnia group. The procedure illustrates the difficulty of conducting a test

[10] M. Smith, A contribution to the study of fatigue, *Brit. J. Psychol.*, 1915–17, 8, 327–350.

[11] E. S. Hermann & S. O. Robinson, Effects of the loss of sleep, this JOURNAL, 1922, 5, 19–32.

[12] E. S. & F. R. Robinson, On the effects of the loss of sleep, *ibid.*, 1922, 5, 93–100.

of the loss of sleep. Many of the control subjects of the experiment did not take the complete series of tests. Moreover, after one has inconvenienced the active subjects by keeping them awake over an unusual period of time, one hesitates to ask that they take tests making severe demands on their energy. This is, as we believe, one of the reasons that the tests used in loss of sleep experiments have been so uniformly short and simple.

In the experiments of Kleitman[13] and of Lee and Kleitman[14] (1923) the two subjects, after remaining awake over periods of 40 to 115 hours found that they could keep awake if they were active, but went to sleep at once upon lying down. Although most of the tests used by these experimenters were physiological, many of them can be included in the field of psychology as well. The experimenters found no changes greater than the daily fluctuations in the tests measuring the amount of blood sugar, the alkaline reserve, the blood-cell count, body-weight, appetite, body-temperature and the ability to do mental arithmetic. There were marked decreases in respiration, heart rate, and the blood pressures (both systolic and diastolic). There was some evidence of a block in the pyramidal system of fibers. The diurnal temperature-wave tended toward effacement, *i.e.*, the temperatures at 1 P.M. and at 1 A.M. approached each other. The differences that these investigators found indicate the trends of such measurements under conditions of loss of sleep, whether or not they are marked enough to be very significant.

In the second part of their experiment, knee-jerk, pupillary reflex, sensory threshold for faradic stimulation, steadiness (a graphic method and the Romberg position), reaction time to auditory and to visual stimuli, naming of opposites, color-naming, cancellation of figures and mental arithmetic were used. Only the pupillary reflex, the steadiness test, and the color-naming showed significant differences. In sleep the knee-jerk disappeared, as has been observed in similar experiments.

Laslett conducted an experiment on the effects of the loss of sleep over a period of fifty hours with tasks involving various levels of mental effort or of complexity.[15] Five subjects and five control subjects were used. Rote memory for digits (as involving the least complexity), code writing (as involving a higher level of complexity), and the solution of analogies (as involving a larger amount of mental effort) were employed. The results showed little or no loss in the rote memory tests; more marked effects in the code writing; and very marked effects in the solution of analogies. The control subjects lost to some extent in the analogies during the sleepless period of the active subjects because of excitement, according to their reports. They did not lose to an appreciable extent in the other tests. The effect of the loss of sleep on the scores in the analogies-test in comparison with those from the rote memory was taken as a confirmation that the longer and more difficult tests are the more valuable in experiments involving the loss of sleep.

Weiskotten conducted a minor study on himself as subject, using a 62-hour period without sleep.[16] He learned nonsense syllables as a test of memory, used addition of numbers as a test of speed, and A-cancellation as a test of accuracy. Speed and accuracy are not inseparable but merely subject to relative emphasis. If a single

[13] N. Kleitman, On the effects of the loss of sleep, *Amer. J. Physiol.*, 1923, 67, 67–92.

[14] M. Lee & N. Kleitman, On the effects of the loss of sleep, *ibid.*, 1923, 67, 141–152.

[15] H. R. Laslett, Experiment on the effects of the loss of sleep, this JOURNAL, 1924, 7, 45–58.

[16] T. F. Weiskotten, On the effects of the loss of sleep, *ibid.*, 1925, 8, 363 ff.

score is used to express the value of the work of a test-period, the one must be converted into terms of the other. Weiskotten used an 8-day practice period, and followed the experiment for five days succeeding the close of the sleepless period. His results showed only slight losses in the memorization of nonsense syllables and negligible losses in the other performances. He states that he believes that loss of sleep affects the mental powers through the diminished power of attention, but does not offer an explanation.

A series of physiological and psychological tests was given at George Washington University by Moss and others.[17] The physiological tests were blood analysis, urine analysis, blood count and blood pressure; the psychological were several forms of the Army Alpha intelligence test, opposites, cancellation, visual acuity, comprehension of visual stimuli and subjective observations on the driving and parking of an automobile. Eight subjects undertook a period of sixty hours without sleep. Two of these extended the period to approximately eighty hours. The results of the tests showed no marked physiological variations although the variations were generally in the direction of loss of ability. The psychological tests showed little loss of ability to perform. These were simple, and in some cases, apparently, of too brief duration. The failure of the experimenters to use a practice period renders comparisons of the results from the various days impossible except to the extent that one may state that the effects of practice were slightly greater than the effects of the loss of sleep. The equality of difficulty of some of the tests used on succeeding days may be brought into question, as well as the abstinence of the subjects from drowsiness and fragmentary sleep, especially during the tests with the automobile.

A review of the psychological experiments on the effects of the loss of sleep seems to show that these effects are much less than one might have believed; that only a few of the performances require sufficient expenditure of energy to outweigh the effects of compensatory effort; and that there is an optimal period of preparatory practice for the various tests, which should be found in order that the practice effects may be largely eliminated and thus removed as interference with the expression of fatigue or loss of sleep effects. The general conclusion from the various experiments is that loss of sleep produces deleterious effects; but the course of its action, its more specific psychological influences, the anatomical structures most affected, and the course of recovery remain almost completely unsolved problems for future experimentation.

Experiment I: The Reduction of Sleep for Five Successive Nights

Two problems were of chief interest when this experiment was undertaken. One was the measurement of the effects of

[17] F. A. Moss & others, A study of experimental insomnia, *George Washington Univ. Bull.*, 1925, 1, 1–24.

a reduced amount of sleep (a sleep-ration) over several successive nights; the other was the critical valuation of the tests used for experiments of this type.[18]

Four subjects underwent a sleep-ration involving a forty per-cent reduction of their usual amounts of sleep for five successive nights. The amount of reduction and the total duration were chosen arbitrarily. The tests used were systolic and diastolic blood pressure, code writing, addition, the pursuit meter, the pursuit pendulum, the ataxiameter and the Thorndike intelligence examination, Part I.

Two of the subjects had had no previous experience with the tests, while two had taken all but the blood-pressure readings for ninety days during the previous spring, but without loss of sleep. The experimenter was the same in both experiments. Throughout the experiment the subjects kept daily records of their various activities on printed forms provided for their use. The items included the amount and estimated depth of sleep, the amount and kinds of physical exercise, the amount of study, the types and times of recreation, the emotional tone of the day as a whole with notes on special disturbances, and any marked disturbance of the daily routine. The amounts of sleep which the subjects took during the sleep-ration experiment were; for A, 4.5 hours a night; B, 5.5 hours; C, 5 hours; and D, 4.75 hours. They all slept 12 to 1 and 5 to 6 a.m. Their periods of sleep were continuous. Each subject subscribed to a statement before a notary, giving the amount of his sleep during the sleep-ration experiment, correct within ten minutes for each day.

Blood pressure. Readings were taken for several days preceding and succeeding the period of sleep-ration as well as during the time of the experiment. They were

[18] The experiments were performed in the Psychology Laboratory at Stanford University. The writer is greatly indebted for suggestions and encouragement to Dr. W. R. Miles who constructed or improved most of the tests used; to Dr. Paul R. Farnsworth and Mr. Leonarde Keeler for assistance with the work; to Dr. E. F. Roth who examined the physical condition of the subjects during the second experiment; to Frederick E. Hines, John A. Leiter, Harold W. Peterson, and Edward C. Tabler for serving as subjects in the first experiment; to Ellis R. Abbe, Edward F. Carlson, Oliver F. Clarke, Wm. A. Cooper, and W. A. Page for serving as subjects in the second experiment, and to Mr. Byron S. Harvey and Mr. L. P. Herrington for serving as control-subjects in the second experiment.

not taken each day throughout the practice period as only an average of the blood pressures of each individual was sought. In the other measurements a practice period of eighteen days was required of the two subjects who were not familiar with the tests and a similar period of nine days of the two who were familiar with the tests. The average and average deviation in systolic blood pressure during the experiment for all of the subjects was 121.7 ± 4.5; in the days preceding and succeeding the experiment 124.6 ± 6.9.[19] The measurements for the diastolic pressure were 61.5 ± 4.7 and 60.4 ± 6.3.[20]

Another method of treating the blood-pressure readings is that of finding the pulse pressure or the difference between systolic and diastolic pressures. The difference between the pulse pressures of the two periods, *i.e.* with regular and diminished amounts of sleep, was not greater than some of the daily fluctuations; *e.g.*, 60.2 for the period of the sleep-ration and 64.2 for the other days. The changes in the systolic pressure were greater than those in the diastolic. While the loss of forty per cent. of the normal amount of sleep does not indicate any seriously detrimental effect in terms of blood pressure, there is evidence of a change that should not, at present, be overlooked.

The code test. The code writing test involved the use of the Johnson-Paschal code, as modified by Dr. W. R. Miles in an unpublished research and consisting of a group of 25 codes each presenting 100 letters of material to be transcoded.[21] A different code was used each day until the 26th day when the series was recommenced in order. In scoring the results of this test two persons working independently checked from scoring stencils the letters as written by the subjects. The number of letters converted correctly into the new code material and the time taken for the entire task were reduced by proportion to the number of letters that would have been transcoded with perfect accuracy in five minutes if the subject had worked at the same rate. This method was used to reduce all of the scores on this test and the addition test to single scores representing both the rate and the accuracy of performance of each subject on each day's task. The average number of letters transcoded correctly in each five minutes for all of the subjects during the sleep-ration period was 159.5, as compared to 162.2 for the four days immediately preceding the experimental period.

Stanford addition test. This test consisted of the addition of approximately 80 problems or a number sufficient to require about five minutes. Each problem consisted of a column of ten digits.[22] The digits *0* and *1* were not used, and there were no runs of three or more repetitions of the same digit in the same column. Twenty-five pages of problems, each containing material for a daily performance, were used successively. The addition test was scored in the same manner as the code test. The average score in terms of number of correct additions per daily trial for all of the subjects during the experimental period was 40.7, as compared to 42.0 for the last four days preceding the experimental period.

The pursuit meter. The pursuit meter requires the subject to equalize a varying

[19] The complete tabular records may be obtained by correspondence with the writer.

[20] C. Landis, Changes in blood pressure during sleep as determined by the Erlanger method, *Amer. J. Psychol.*, 1925, 73, 551–555.

[21] H. M. Johnson & F. C. Paschal, Psychological effects of the deprivation of oxygen, *Psychobiol.*, 1920, 2, 193–236.

[22] F. G. Benedict, W. R. Miles, P. Roth & H. M. Smith, Human efficiency and vitality under prolonged restricted diet, *Carnegie Inst. Wash. Publ.*, 1921, 280, 77–105.

current running through a wattmeter by means of a rheostat one of whose poles is in a hard rubber runner that is slid in a groove in the face of the rheostat while the subject is performing the experiment.[23] The adjustments of the rheostat must be continuous. If the subject is to succeed well, it must follow immediately on the heels of any change in the resistance unit. The score for the daily task for each subject is read from the dials of meters which measure the leak in the current, that is, the amount of current which escapes because the subject has not kept the two circuits, one of which is under his control, in balance. The scores are influenced by the quickness of perception for slight movements of a pointer and by the speed and muscular control of the subject in sliding the key over the face of the rheostat. The average score for all of the subjects during the experimental period was 339.8 in comparison with 362.2 for the last four days preceding the experimental period. (The smaller score indicates an improved performance.)

The pursuit pendulum. The pursuit pendulum[24] differs from the pursuit meter in that it provides for a series of discreet and isolated acts, except insofar as a rhythm of operation may be established. The largest possible score in the pursuit-pendulum experiment was 1,250. The average score of all the subjects during the sleep-ration period was 941.3 in comparison with 908.7 during the last four days preceding the sleep-ration period. (A larger score would indicate an improved performance.) It is obvious from a study of the curves that practice effects were still strongly operative. The curves for the days succeeding the sleep-ration period rose sharply in contrast with their small rise during the sleep-ration period.

The ataxiameter. One of the most simple and at the same time most complicated muscular adjustments of the human organism is that of standing with minimal sway of the body. The ataxiameter makes possible the measurement of the amount of sway of a subject in each of four directions, posterior, anterior, to the left and to the right. The scores are in terms of mm of movement of the subject's head.[25] The averages for the second two-minute interval of the experimental period only are given because the writer feels that the second interval represents more accurately the true reactions of the subject to the test. The subject was inclined to hold himself in a stiff and awkward position during the first interval, was able to call to his assistance various cues which might serve him well or ill, and was much more variable in his records. As a result these records for the first two-minute interval are shown in the graphic records alone. The average score for all of the subjects during the sleep-ration period was 364.3 mm in comparison with 347.0 mm for the last four days preceding the sleep-ration period. (The smaller score indicates improved performance.)

The records of the subjects in the forward and rearward directions for the second two-minute interval of the test show that the scores are larger than those for the lateral sway because of the muscle habits involved in walking and running and because of the anatomical structure of man. The average score for all of the subjects during the experimental period was 632.3 mm of sway in comparison with 596.4 mm for the four days immediately preceding. (The smaller score indicates improved performance.)

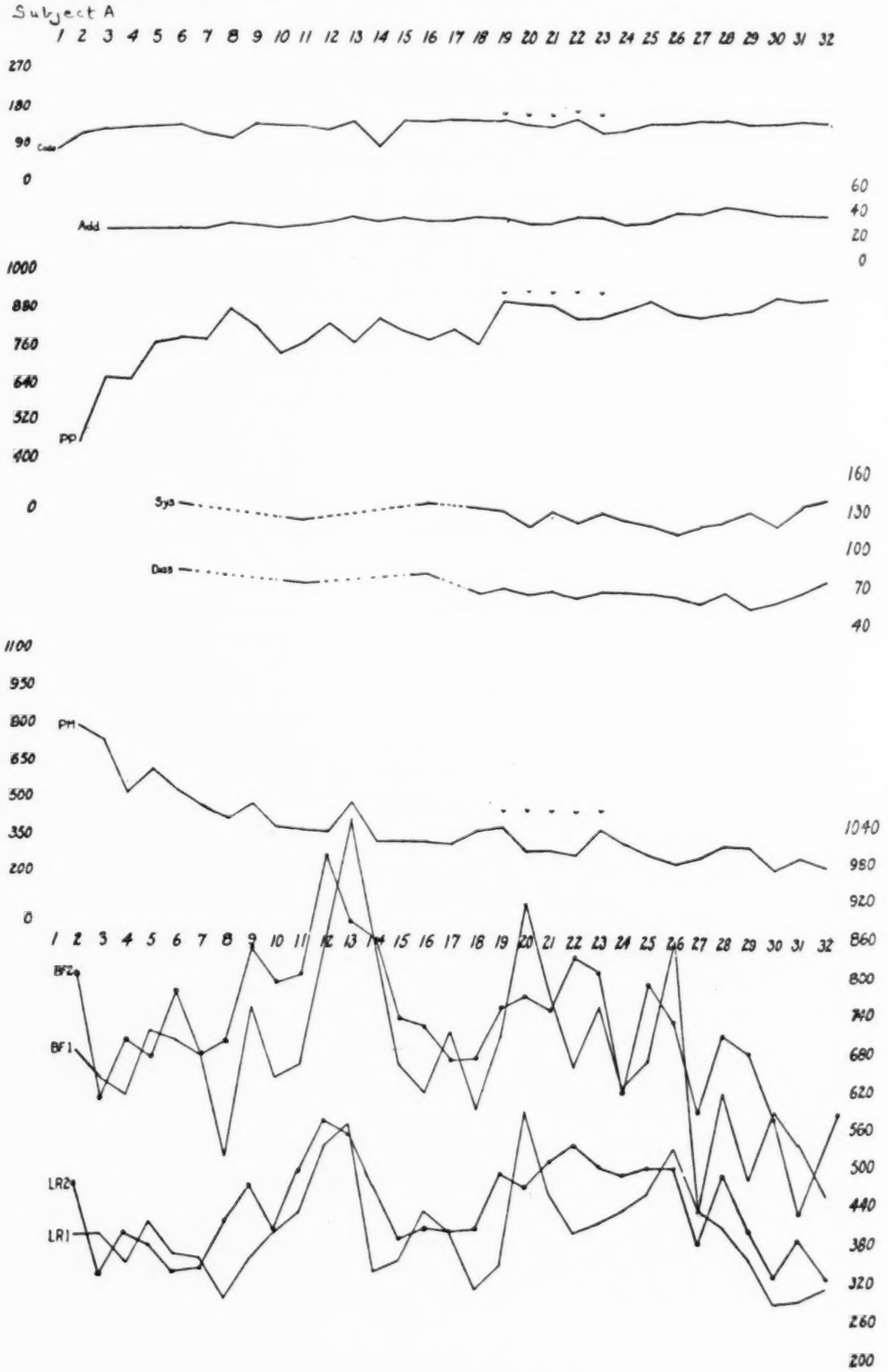
The Thorndike intelligence examination, Pt. I. The scores which the subjects made

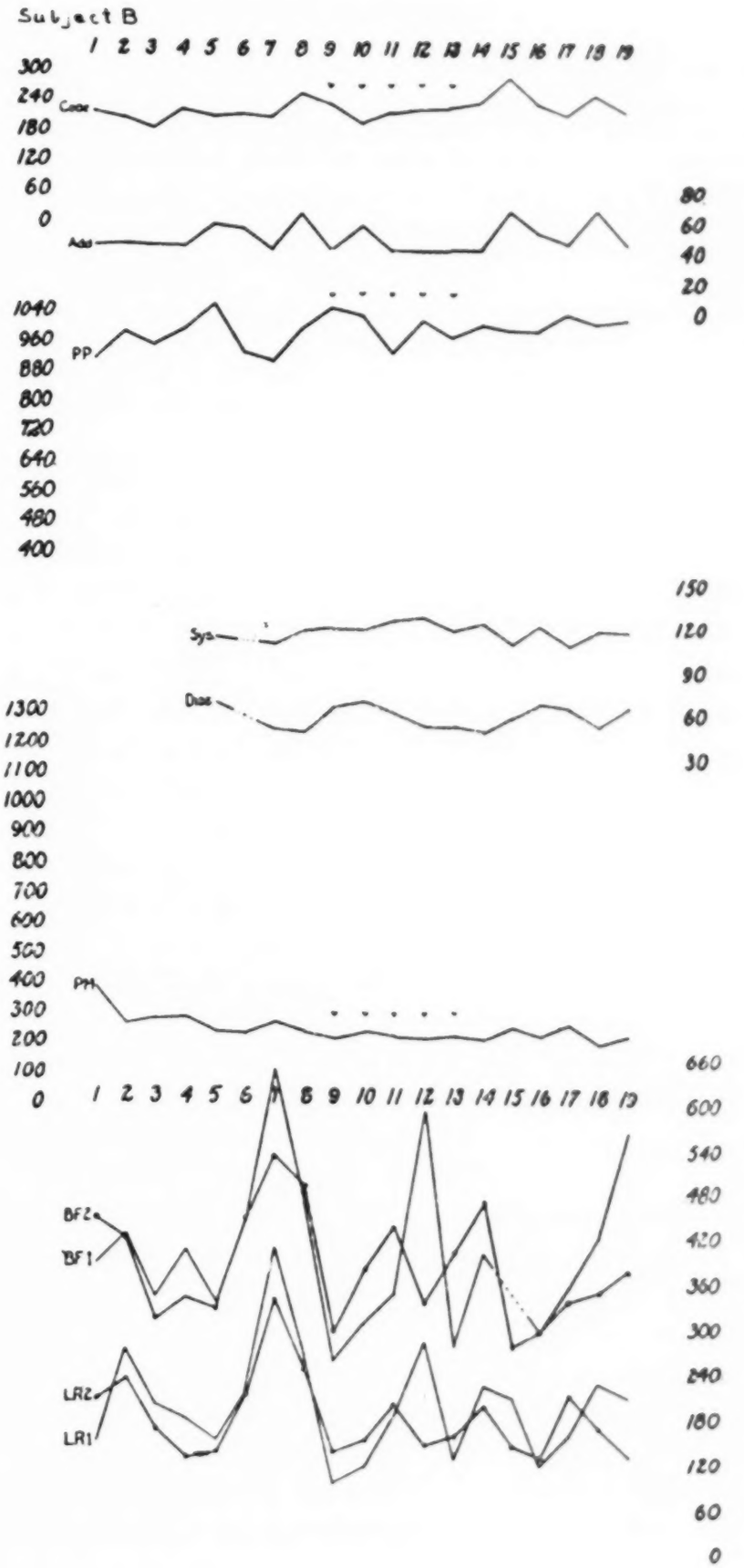
[23] For a complete description see W. R. Miles, The pursuit meter, this JOURNAL, 1921, 4, 77–105.

[24] W. R. Miles, A pursuit pendulum, *Psychol. Rev.*, 1920, 27, 361–376.

[25] W. R. Miles, Static equilibrium as a useful test of motor control, *J. Indust. Hygiene*, 1922, 3, 316–331.

Detailed records (Subj. A and B) for code, addition, pursuit pendulum, blood pressure, pursuit meter and ataxiameter.




Subject B
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19
300
240
180
120
60
0
Cook
Add
80
60
40
20
0
1040
960
880
800
720
640
560
480
400
PP
150
120
90
60
30
Sys
Dias
1300
1200
1100
1000
900
800
700
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500
400
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100
0
PM
660
600
540
480
420
360
300
240
180
120
60
0
BF2
BF1
LR2
LR1

at the close of the sleep-ration were compared with the scores which they had made on a similar form of the same examination at the times of their admission to Stanford University, modified by the subtraction of a practice increment. Because the number of cases in the retest experiment by which the practice increment was found was small, the writer desires to call attention to the probable error of the practice-effect found. If the score made on Part I of the examination at the time of admission to the University is taken as 100 per cent. for each subject, the modified scores made at the close of the sleep-ration period are; 80.1 per cent., 82.3 per cent., and 93.0 per cent. The average loss in terms of percents of score is 14.9 per cent. The writer believes this to be significant. The scores of the fourth subject could not be compared because of the difference in the examination-form used at the time of his admission to the University and that used in the experiment. None of the subjects had taken Form N before the experiment.

Conclusions. The writer believes that these tests were varied enough and difficult enough to justify their use as measures of the effect of a sleep-ration involving a 40 per-cent reduction in the customary amount of sleep of each subject. Care was taken throughout the experiment that the subjects did not learn their scores. They were instructed to do their best from day to day and were warned that their opinions of the value of the work done in the various trials were not valuable as indices of performance, in order that they might be influenced as little as possible by them.

The sleep-ration which involved a 40 per-cent reduction from the normal amount of sleep for five successive nights produced a noticeable effect upon the scores of the subjects in nearly all of the tests. The effects were most noticeable in the pulse-pressure and the blood-pressure readings, the code test, the addition test, the pursuit meter experiment, the ataxiameter experiment (both lateral and anterior-posterior directions), and the Thorndike intelligence examination, Part I.

The scores obtained from all of the tests together do not yield readily to statistical treatment; but analysis of the scores and the graphs shows noticeable losses in nearly all of the tests by nearly all of the subjects during the sleep-ration period, especially if one considers the sharp rises in the curves following the close of the period of diminished sleep. There was no evidence of a recovery spread over a series of weeks or days though the tests were carried on for two weeks after the close of the sleep-ration period. One night of the

usual amount of sleep seemed to serve to rest the subjects from their experience. There was no plateau even after recovery because the practice effects were still operative. The only difference in the subjects during the sleep-ration period noticeable to the writer was a tendency to be more talkative and excitable. One subject had an auditory illusion, that of hearing the University chimes at a distance at which they could not have been heard. This was the only illusion in either experiment.

Experiment II: The Loss of Sleep over a 72-Hour Period

In the second experiment the subjects were expected to remain awake for seventy-two consecutive hours. The original plan had been that four subjects and four control subjects would take part, but two control subjects were compelled to withdraw after the experiment was under way, and none of the active subjects cared to become control subjects after the practice had begun. The tasks of the active and the control subjects were identical except for the abstinence from sleep during the three-day period. The practice period of the experiment was begun January 21st and continued until February 19th. The test period, during which the subjects had no sleep, covered the dates February 19, 20, 21. Two of the subjects carried on two of the tests for a period of three weeks after the close of the sleepless period in order to measure any fluctuations in recovery that might occur.

In this experiment the same tests were used as in the sleep-ration experiment; blood-pressure readings, code writing, addition test, the pursuit meter, the pursuit pendulum, the ataxiameter, and the Thorndike intelligence examination, Part I.

In addition to these, studies of the pulse rate, the sub-lingual temperature, visual acuity by means of the device devised by Ives, Cobb and others, measurements of the speed and accuracy of eye movements as recorded on a photographic plate by a special camera, the Lowell modifica-

tion of the Snellen letter charts, and an astigmatism chart were included.

The Ives-Cobb visual acuity test. The experimental use of the Ives-Cobb device [26] consists in the discrimination of a series of parallel lines at their narrowest appreciable width. The direction in which the lines run may be changed to any angle with the horizontal. The chief use of the changed direction of lines is that the experimenter can be assured that the subject sees the lines and that his imagination does not cause him to believe that he sees them before he does. The daily task was sixteen readings. The time required was from five to six minutes for each subject daily. The score is in terms of revolutions of the micrometer adjustment screw from a zero position.

The eye camera. The camera for photographing the horizontal movements of the eye, as we planned to use it, involves the same principles as smaller cameras, except that a Dodge falling-plate carrier and a timing device, developed in part at the Marey Institute, were used.[27] The falling-plate carrier carries the plate downward past the area of exposure at a determined rate of speed so that the horizontal movements of the eye, as shown in the form of dots obtained by the interruption of a beam of light reflected on the edge of the cornea, may be recorded. Because the plates depended on for this experiment proved to be too inert for the work, no consistent data were obtained. The writer believes the experiment has great promise in connection with the loss-of-sleep experiments, and has, for this reason, included mention of it here. Descriptions of the other tests are not necessary as they are familiar to everyone. The writer placed special stress upon tests of vision because he believed that sleep might be closely related to ocular reactions. This did not prove to be the case with these tests. More delicate tests may show effects of insomnia upon vision.

The technique on the effects of want of sleep over a seventy-two-hour period was the same as that employed in the sleep-ration. Throughout the period of the experiment the subjects kept daily records of their activities outside of the laboratory similar to those used before. During the course of the 72-hour period without sleep the subjects spent the first day in their accustomed manners, but reported to the laboratory in the evening. The first night was spent in study, conversation, games and short walks. The second day the subjects attended classes and followed other activities,

[26] F. G. Benedict, W. R. Miles, P. Roth & H. M. Smith, Human efficiency and vitality under prolonged restricted diet, *Carnegie Inst. Wash. Publ.*, 1919, 280, 143–144.

[27] R. Dodge & F. G. Benedict, Psychological effects of alcohol, *Carnegie Inst. Wash. Publ.* 1915, 232, 151 ff.; R. Dodge & Cline, The angle velocity of eye movements *Psychol. Rev.*, 1901, 8, 145 ff.; W. R. Miles, Alcohol and human efficiency, *Carnegie Inst. Wash. Publ.* 1924, 333, 16–28; W. R. Miles, Human efficiency and vitality under prolonged restricted diet, *ibid.*, 1918, 266, 81 ff.; W. R. Miles & E. Shen, Photographic recording of eye movements in the reading of Chinese in vertical and horizontal axes, this JOURNAL, 1925, 8, 344–362.

but spent much of the time under observation in the laboratory. The second night was spent in much the same manner as the first except that study proved so unprofitable that the attempts were abandoned before midnight. During both of these nights the experimenter was with the subjects. On the third day the subjects spent part of the morning away from the laboratory in the pursuit of active exercises, but in the afternoon all reported to, and remained at, the laboratory until the time of dismissal on the final morning. During this period they were under the observation of the writer, Dr. W. R. Miles and Mr. Leonarde Keeler. The second night, rather than the third, was the most difficult because on the latter the end of the experiment was in view.

The Thorndike intelligence examination was commenced at 6:30 a.m. and was succeeded by the other tests in their usual order. The subjects went to their rooms with instructions to sleep as long as they wanted to. The longest period of uninterrupted sleep was eleven hours. The average was just less than nine hours. This inability of subjects who have been without sleep for some time to sleep for unusual periods of time has been found by a number of experimenters.

Each subject signed a statement before a notary that he did not obtain any surreptitious sleep during the 72-hour period while he was away from the laboratory. Since all of the subjects were keenly interested in the experiment and were watched by each other as well as by the experimenter, we are sure that they did not obtain any sleep at the laboratory.

Blood pressure. The procedure was standard. The measurements were taken for several days preceding and succeeding the testing period. If the time that the subjects had been able to spare for the experiment had been greater, more attention would have been paid to blood-pressure readings, cell count and other physiological factors. The systolic pressures fell, on the whole, throughout the sleepless period until the last reading, when four of the five subjects showed a rise of pressure. The subjects were more variable here than the control subjects. In four cases out of the five for the active subjects the diastolic pressures were higher during the sleepless period than they were in the preceding and succeeding days. The fluctuations from the averages of the normal period and the fluctuations from day to day were also much larger. The average systolic pressure for the active subjects during the sleepless period was 118.0 as compared to 120.3 for the days outside of this period. The average diastolic pressure for the active subjects during the sleepless period was 67.1 as compared to

63.7 for the preceding and succeeding days. Thus the loss of sleep tended to decrease the difference between the pressures. The pulse-pressure readings show an increasing change within the duration of the sleepless period. The difference between the systolic and diastolic readings [28] is as follows:

Days with sleep	Days without sleep
51.3	49.6
63.2	59.8
53.6	47.6
64.3	46.8
55.0	
57.5	50.9

The pulse-pressure difference between the sleepless days and the others is a difference that can not be overlooked, because it indicates a disturbed physiological condition, primarily an overload on the heart, but a condition which deserves study in itself in order that the exact physiological disturbance may be found. That the difference in this case is due to loss of sleep we see no reason to doubt, since all other known factors were equalized.

Code test. In this test the procedure was identical with that in the sleep-ration experiment. The results were scored in the same manner as in the earlier test. This test was carried on by Subjects 8 and 9 for fifteen days after the end of the sleepless period in an attempt to observe any fluctuations of recovery that might occur. While there were fluctuations of performance these were different for the two subjects and were due more to fluctuation of interest and to the pressure of other activities than to other causes. The following are average differences between the sleepless period, the three days immediately preceding, and the fifteen days succeeding:

THE CODE TEST

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days [29] immediately preceding	135.3	108.3	107.0	112.0	162.0	118.0	158.7
Ave. test days	114.3	91.0	92.0	114.0	146.0	125.7	167.0
Ave. 15 days succeeding				120.3	159.8		

The averages of all of the subjects were lower during the sleepless period except that of Subject 8. The deviations were higher except those of Subject 9.

Addition test. In this test the procedure was identical with that in the sleep-ration. All of the subjects solved fewer problems during the sleepless period than they had solved in the days preceding; but some of the subjects were more affected than others. The average deviations during the sleepless period were greater than during the days preceding. The average for all of the subjects except the control subjects for all of the days except the test days and the first four practice days was 22.2 problems; while for the test days it was 20.1 problems per work-period. The averages for the subjects follow:

[28] The pulse-pressure measurements are not given for the control subjects because of the small number of readings.

[29] A larger score indicates an increased skill in performance. (c) = control subjects.

THE ADDITION TEST

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[29] immediately preceding	21.7	32.7	17.3	13.0	35.3	21.0	26.3
Ave. test days	17.7	25.7	13.0	11.7	32.3	20.7	28.7

The pursuit-meter experiment. The procedure in this experiment and the method of recording the scores were the same as in the sleep-ration. In this test two of the subjects (7 and 9) continued to improve in their performances during the sleepless period but not at the previous rate. One of the control subjects improved markedly during the three days of the sleepless period while the other showed as much loss as the active subjects. The averages are:

THE PURSUIT-METER

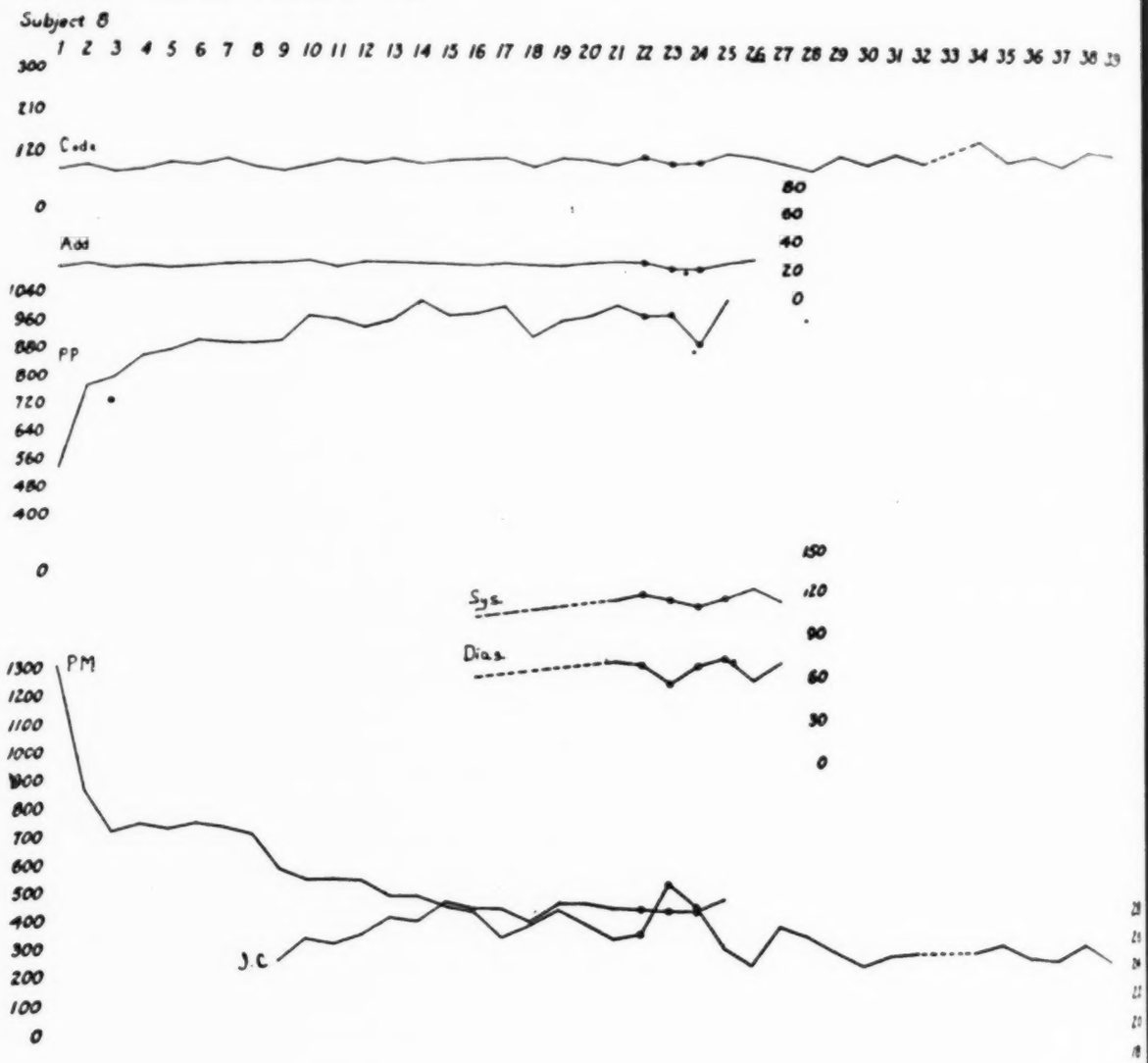
Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[30] immediately preceding	468.0	406.0	609.3	420.3	349.7	458.7	567.0
Ave. test days	575.7	472.0	663.0	478.3	320.3	355.0	625.3
Ave. 15 days succeeding				331.4	249.1		

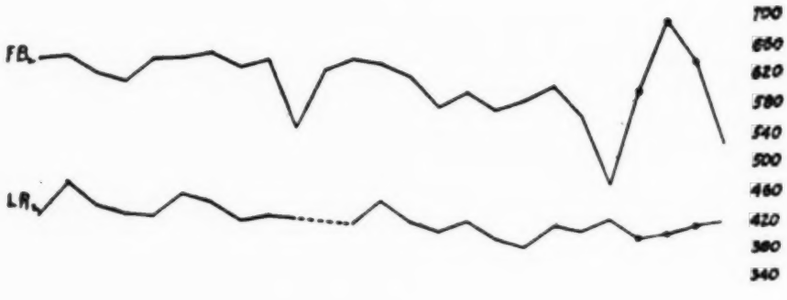
A study of the individual curves of the subjects, especially 8 and 9 who continued their tests for fifteen days after the close of the sleepless period, shows that the effects of practice were still strongly operative. In spite of this fact three of the subjects did not maintain their levels of performance though two of them showed increased skill during this period. A study of the graphic records of the subjects on the pursuit-meter to determine whether those who made larger scores (less skilled in terms of performance) made them through numerous small increments or occasional large increments shows (1) that for the first minute of the experiment during the sleepless period the subjects did about as well as usual although the fluctuations were a little wider and the subject's responses a little slower, (2) that the introduction of wide variations of performance was gradual, and (3) that at the close of the five-minute period the subject was doing comparatively little toward consistent control of the pointer-arm. A longer practice period and a longer test period promise a great deal for the usefulness of this experiment in connection with loss of sleep observations.

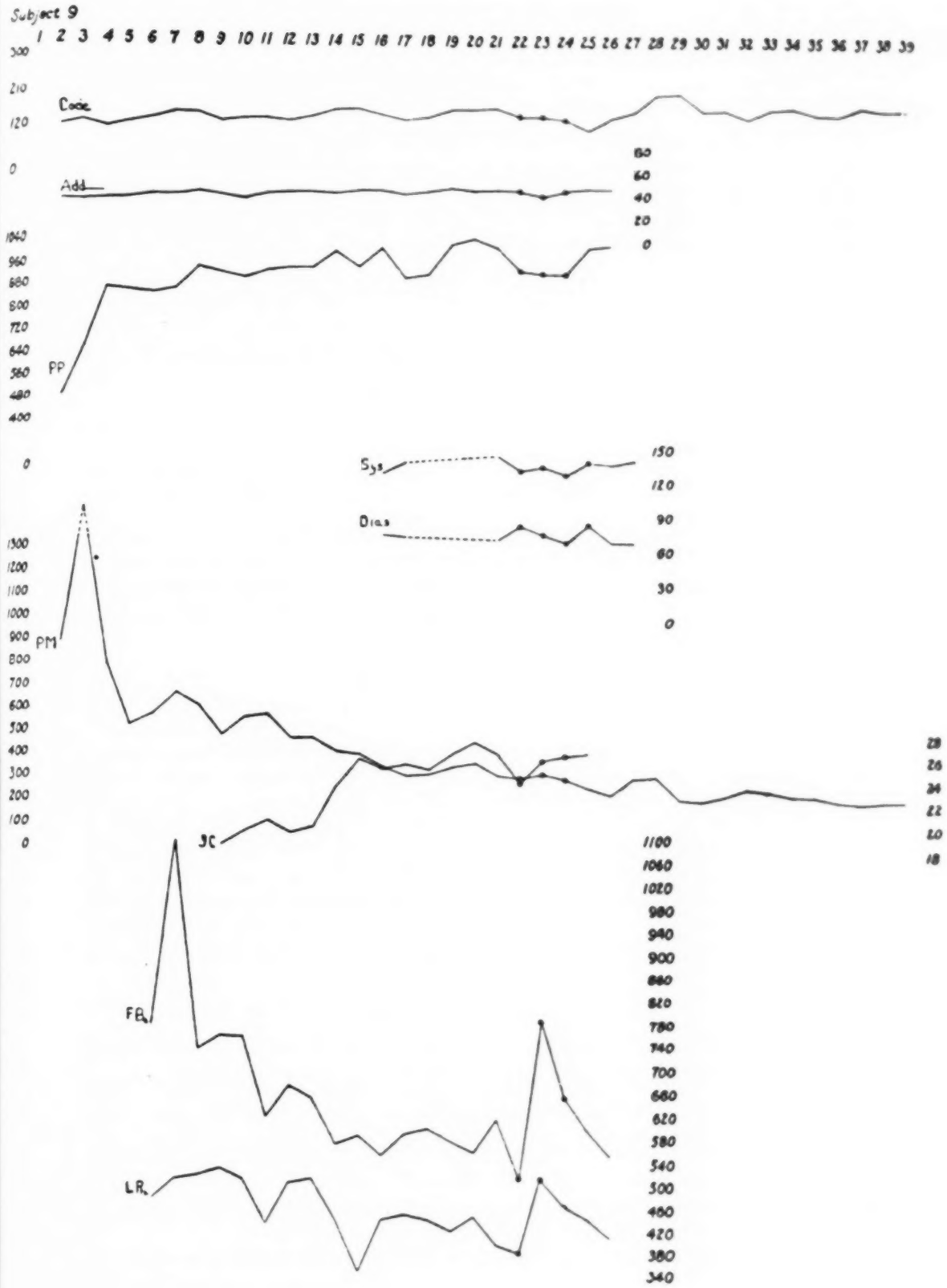
The ataxiameter test. Procedure and method of record were the same in this experiment as in the sleep-ration. The lateral sway is affected much less by training than the anterior-posterior sway in the results of the experiment. The amount of sway of all of the subjects was larger during the sleepless period than in the days preceding. In the following table the symbol (y) refers to the performance of Subj. 6 on the last test, given during the last hours of the sleepless period. While he was being measured in the ataxiameter, he drowsed and nodded so hard during the two minutes of quiet and monotony that he pulled the threads and weights which turned the dials over the pulleys on which they rested and displaced the apparatus which was already approaching its capacity to register. The scores for the first and second days of this period were used in finding his average score for the insomnia period. The score is smaller than it would have been if the third day had been included; but the difference is so large anyway that the effects of the loss of sleep are clearly evident.

[30] A smaller score indicates an increased skill in performance. (c) = control subjects.

Detailed records for Subj. 8 and 9.






Subject 9
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39
Cocic.
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LATERAL SWAY ON THE ATAXIAMETER

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[31] immediately preceding	205.3	328.7	566.7	219.0	241.3	184.3	258.7
Ave. test days	356.7	718.3(y)	564.0	208.7	290.7	209.0	312.3

The average score for all subjects except the controls for the period preceding the sleepless days (excepting the first four days) was 310.6 mm of sway. For the sleepless period it was 427.7 mm of sway. The effect of the loss of sleep was greater than the practice effect, even though this was still operative as was indicated by some later experiments with two of the subjects with the ataxiameter.

Posterior-anterior sway. The effects of the loss of sleep on the lateral sway were not so great as on the posterior-anterior sway. The scores of all subjects increased sharply on the first day of sleeplessness with the exception of Subj. 9. On the third day they were all greater than the average of the days preceding the sleepless days.

POSTERIOR-ANTERIOR SWAY ON THE ATAXIAMETER

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[32] immediately preceding	586.7	434.3	940.0	416.3	485.0	509.0	573.0
Ave. test days	955.7	929.5(y)	1162.0	570.7	588.3	493.7	563.3

The pursuit pendulum. Procedure and method of recording same as in the sleep-ration experiment. The scores were smaller during the sleepless period; but the losses, when compared with the preceding scores plus the average deviations, were small enough to fall within the deviations. In the cases of Subj. 5 and 7 the losses were negligible. The influence of practice was still strong.

THE PURSUIT PENDULUM

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[33] immediately preceding	1005.7	1003.0	965.7	1009.3	1034.0	969.3	1004.7
Ave. test days	1001.7	931.7	935.0	971.3	937.3	955.7	998.3

The Ives-Cobb test. The Ives-Cobb test consists in the discovery of the point at which the subject can see and determine the direction of a series of parallel lines that are increased in width from a degree of fineness that renders them invisible under the conditions until they can be seen. Each score is the average of the sixteen trials that comprised the daily task. These varied little from trial to trial even as the averages varied little from day to day. During the sleepless period most of the subjects made lower scores (*i.e.* improved in terms of performance).

THE IVES-COBB DEVICE

Subjects	5	6	7	8	9	10(c)	11(c)
Ave. 3 days[34] immediately preceding	2.767	2.684	2.763	2.788	2.621	2.748	2.697
Ave. test days	2.756	2.508	2.742	2.748	2.534	2.784	2.739

[31] A smaller score indicates an increased skill in performance. (y) = average score for the first and second days of the test period only. The third score was lost through the extreme nodding of the subject. (c) = control subjects.

[32] A smaller score indicates an increased skill in performance.

[33] A larger score indicates an increased skill in performance. (c) = control subjects.

[34] A smaller score indicates increased skill in performance.

The average for all of the subjects for all of the days preceding the period of loss of sleep except the first four practice days and excluding the results of the control subjects was 2.673. For the sleepless period the average reading for the subjects was 2.658. This difference does not seem significant.

The Thorndike intelligence examination, Pt. I. The results will be stated for each subject as percentages, taking the average of the score made in the University entrance examination and the score made before there was any loss of sleep as 100 per cent.[35] The second score was corrected by the subtraction of an amount found (approximately) by the writer by giving these same tests in the same order to a group of seven students from the same collegiate class as the subjects in the experiment. The students to whom these tests were given were not average students as their scores in the early tests were considerably higher than the average scores on this part of the test. Since many of them had a sufficient amount of time to check their work on the test once or even twice, the writer believes that the losses shown by these subjects in the sleep experiments are less than the true losses.

The scores in terms of percentages appear below.

Subject	Entrance test	After 56 hours (% of accomplishment)	After 72 hours (% of accomplishment)
5	100	28	61
6	100	86	77
7	100	95	57
8	100	102	106
9	100	93	76
10(c)	100	123	138
11(c)	100	121	123
Ave. of Subjects (minus practice effects)		80.8%	75.4%
Ave. Loss		19.2%	24.6%

A large number of daily observations were made with each of the subjects on the Lowell modification of the Snellen Letter Chart for vision, and on a standard chart for astigmatism. Three forms of the letter charts were used in a chance order. All lines of letters above those which offered some difficulty of reading were covered with a white cardboard to avoid the unintentional identification of the chart and the memorization of the letters by the subjects. No noticeable changes were produced by the loss of sleep in either of these tests.

Summary

A study of the sleep-ration experiment, which involved a forty-per-cent reduction in the amount of sleep for five consecutive nights, shows that the effects of such a reduction are not pronounced, as indicated by the tests used. The Thorndike intelligence examination shows the most noticeable effect.

The recovery of the subjects, if there could be a recovery

[35] The use of Part III instead of Part I is recommended as being more difficult and as requiring more sustained effort even though the units of measurement are not so fine as in Part I.

from such a situation, was apparently complete after two or three hours of extra sleep. These results might have been different with other types of subjects who had less vitality and less youth. Much more experimentation must be done in the relation of sleep to many other physiological and psychological factors before results that now appear to be inconsistent may be found reconcilable. Among these is the nearness of approach of the usual day's work to the limit of the energy of the individual in comparison with the moderate day's work that is customary with other individuals, in its relation to the need or the desire for sleep. The writer believes that the field of the reflexes should be studied as they give promise of valuable results, especially since (as we may assume) they are little influenced by compensatory effort.

In the experiment involving a period of seventy-two hours without sleep, the subjects showed losses in performance on the following experimental tests sufficiently large to warrant their continued use in similar experiments:

	Loss (%) in test period in comparison with 3 days immediately preceding
Pulse-pressure readings	14.8
Code test	10.7
Addition test	13.8
Pursuit-meter	8.8
Ataxiameter: lateral	33.8
posterior-anterior	51.8
Thorndike test (Pt. I)	24.5

The tests which showed variations so small that they were considered negligible were

The pursuit pendulum	4.8%
The Ives-Cobb test	2.5%
Snellen Letter Chart	no change
Astigmatism chart	" "
Pulse	" "
Temperature	" "

The tests involving the use of vision were a surprise to the experimenter as he believed, from general observation, that the relation of want of sleep to the loss of visual power was close. Apparently there is a close relationship only

between the want of sleep and ocular discomfort, and little between ocular discomfort and visual acuity and perception.

The amounts of sleep taken to recuperate from this sleepless period were short. Eleven continuous hours of sleep formed the longest period taken by any subject. The average was nine, although the subsequent two or three periods were, in some cases, lengthened from thirty to sixty minutes. One of the subjects felt a certain loss of equilibrium after the first sleep-period succeeding the experiment. This lasted only a few moments and was not serious enough to cause more than slight discomfort. Whether this is of any significance in the physiology of such experimental procedure is a question of some interest.

Two subjects continued two of the tests (code and pursuit-meter) for fifteen days after the close of the sleepless period to note any fluctuations of recovery. The results do not show noticeable fluctuations which can be ascribed to causes other than fluctuating interest and pressure of effort in other lines of work. Subjective estimates of this period seem to show complete recovery after nine to eleven hours of sleep, and the scores confirm this estimate.

Comments of the subjects upon the pursuit-meter, the pursuit pendulum and the Ives-Cobb tests suggest that the subjects were impressed with the small part apparently played by consciousness in their efforts during the sleepless period. They responded to the stimuli although they were barely aware of them. Their performances were more highly automatic than we might expect from their degrees of practice.[36] The subjects thought that they had responded sometimes without being aware of the stimuli.

[36] A difference between the effects of the loss of sleep and those of anoxemia and anæsthetization is that in the loss-of-sleep state the muscles operate although consciousness is not alert; while, in the latter, consciousness may be fairly alert (or seem to be) and yet the muscles fail to operate. A difference between sleep and the reactions of the body to certain drugs is that one can overcome the effects of sleepiness for various periods of time in response to interest or necessity. When they are being absorbed by the body drugs continue their chemical effects in spite of the necessity for action or the interest that may seem advisable. Sleep is more flexible or spasmodic; drugs must run their courses when they are in the body. An added difference between the effects from the loss of sleep and alcoholic intoxication lies in the fact that in the former state visual capacity is little affected while in the latter it is noticeably altered.

One case of hallucination was reported during the sleep-ration. None was reported in the second experiment.

From time to time various writers have suggested that the time given to sleep could be shortened by hastening the process in its depth. They have maintained the opinion that, since the first two hours are supposed to include the deepest sleep, they are the most valuable. Their conclusions are open to criticism. The first hours of sleep are not necessarily the most valuable even if deepest. The greatest mental and physiological accomplishments are not necessarily the most effortful; nor is interrupted sleep, which might introduce this deepest sleep in several successive periods, apparently the most restful. All psychological and physiological activities have their own optimal rates of reaction. Any considerable decrease in the reaction time of these may make for confusion and unnecessary waste effort rather than increased accomplishment over a period of time longer than a few moments. Whether the increased amount of time gained from shortened periods of sleep would be time saved for work is questionable, since work has rate and accuracy as well as duration. Whether the discomfort caused is compensated for in the satisfaction of work completed or time spent at work is somewhat an individual matter. Perhaps we shall come to measure compensatory effort in terms of the ratio between the work one can compel himself to do under pressure and the work that one would do at a normal rate over a fairly extensive period of time.

The small amounts of sleep customary to men noteworthy in achievement are probably mythical. The amount of sleep which an individual needs is, apparently, subject to large degrees of variation, according to the rate of metabolism and the energy-drive of the individual. The advocacy of smaller and smaller amounts of sleep is dangerous when disseminated as authoritative, rather than speculative, to people who are not able to judge it carefully. The pursuit-meter experiment showed that the subjects who had been without sleep for some time reacted to the device very slowly and often extremely. Their reactions were not complete, but

seemed only attempts to react. With the ataxiameter the subjects did not bring their bodies back to the perpendicular when they were only slightly out of the erect position, but waited until they were markedly out of balance before they reacted. Such slow and incomplete, or exaggerated, responses, especially when complicated by the loss of much ability to solve problems and to form conclusions, points to the desirability of a sufficient amount of sleep for those people who have to make quick reactions based on judgments that are reached quickly and accurately.[37]

1. It is probable that sleep can be initiated under conditions which are preponderantly physiological or psychological, according to the circumstances preceding the particular occasion.

2. The longer tests in the experiments show, as a rule, the greatest losses. Tests should be long enough to negate the marked effects of compensatory effort, but not long enough to cause fatigue themselves. The practice series should be long enough to eliminate the marked effect of continued practice.

3. The effects upon the scores of tests from a sleep-ration involving a forty-per-cent. reduction of the normal amount of sleep for five successive nights are sufficient to show losses in ability to perform these tests although the losses were not so great as those in the experiment during which the subjects obtained no sleep.

4. Experimental tests that should be included in similar experiments in the future are pulse pressure, the code test, the addition test, the pursuit-meter, the ataxiameter and an adult intelligence examination.

5. The belief that the amount of sleep which an individual should have can be curtailed or eliminated without loss of

[37] An opinion which appears to be of some interest was reached during the course of the latter experiment when it was too late to study it at that time, *i.e.* that in long periods without sleep the attacks of sleepiness come in cycles at approximately three-hour intervals. This might explain in part the fact that some of the subjects made better performances on later tests than on earlier ones in the sleepless period. If this observation is shown to be correct, it might contribute to the theory of sleep as one of the instincts.

efficiency is not borne out by experimental evidence. The amount is an individual matter, conditioned by a large number of factors in the environment of the individual, but more or less common to all, and conditioned further by the reacting ability of the organism itself.

6. It is possible that sleep-seizures during long periods without sleep come in cycles similar to those of hunger.

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DAY AND NIGHT INTERVALS AND THE DISTRIBUTION OF PRACTICE

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It is a well-known fact that the time required to memorize material is considerably influenced by the introduction of rest-periods during the process of learning. The present investigation is concerned with the efficacy of these rest-periods as dependent upon the nature of the subject's activities therein. We propose to compare the relative efficacy of two 12-hour rest-periods which were inserted at the same stage of learning, one of them occurring during the day and the other during the night. In one the subject was occupied with daily routine, while the major portion of the other was devoted to sleep.

A group of 51 individuals participated in the experiment. The material to be learned consisted of eight lists of 14 paired words, presented on a memory drum at the rate of 3 sec for each pair. The subjects were tested for recall after each presentation, the experimenter recording the number of items correctly reproduced. Each list was learned in two sittings, separated by an interval of 12 hours. During the first sitting the material was presented until the subject was able to recall at least half the items. Then the learning was completed 12 hours later. At the end of the first sitting the subjects were asked not to revert to the material during the interval of rest. The time of the test varied with the individual within the time limits 7–10 A.M. and 7–10 P.M.; but the interval between the two sittings was approximately 12 hours for each individual.

The subjects were divided into groups of 25 and 26 equated as to age, training and memorial ability. All subjects learned the same eight lists; but the order differed for the two groups to equalize practice. Individual differences were cancelled because all the individuals appeared an equal number of times both day and night; differences in the difficulty of the lists were equated because each list was learned by practically the same number of subjects for both conditions, and differences in diurnal efficiency were equated in part because each list was learned half in the morning and half at night.

Table I gives the mean number of trials per individual per list for each of the two conditions, the probable errors of the two means, and the P.E. of their difference. Each mean is based upon 204 measures, one for each of four lists and 51 subjects.

TABLE I

TRIALS REQUIRED FOR LEARNING

Mean for total learning; *rest during day*	5.52
P.E. of mean	.117
Mean for total learning; *rest during night*	4.93
P.E. of mean	.10
Difference between the means	.50
P.E. of the difference	.15

The difference between the time required to learn with rest during the day and that with rest during the night is more than three times its P.E. We conclude, therefore, that a rest-period in which the major portion is devoted to sleep is more effective than an equal interval during which the subject pursues his daily routine of activities, *i.e.* that a complete rest is more beneficial than is a change of occupational activities.

As a check upon the validity of the conclusions the results for the two halves of the learning are presented separately in Table II, which gives the mean number of trials per individual per list required for the half-learning periods for both day and night intervals.

TABLE II

TRIALS REQUIRED FOR EACH HALF-PERIOD OF LEARNING

Mean for 1st half of learning; *rest during day*	1.86
Mean for 1st half of learning; *rest during night*	1.89
Difference between the means	.03
Mean for 2d half of learning; *rest during day*	3.66
P.E. of the mean	.09
Mean for 2d half of learning; *rest during night*	3.02
P.E. of the mean	.078
Difference between the means	.64
P.E. of the difference	.12

The results for the initial period of learning are practically identical for the two conditions. After the rest interval, however, there is a significant difference in the rate of learning. This difference must be a function of the character of the two intervals and not of the time of day, for otherwise we should expect to find a corresponding difference for the first period of learning. As a matter of fact, the difference in the first half stands against the night learning. Finkenbinder's results also indicate that there is no appreciable difference in diurnal efficiency for the hours in which our experiment was conducted. His results indicate that learning at 8 A.M. was less than 4 per cent. more effective than at 8 P.M.[1]

Our results are also in harmony with those of Dallenbach and Jenkins, who investigated the relative effect of two such intervals upon the ability to recall that which had previously been completely memorized.[2] They found that after eight hours of sleep the subjects reproduced over twice as many syllables as after an equal interval during which the subject had been awake.

Our results are also significant from the standpoint of theory. It is usually assumed that the introduction of such an interval facilitates learning. Does the subject indulge in a memorial review of the material during this interval, or is the facilitating process wholly physiological? In as much as facilitation occurs during sleep, and to a greater degree than while awake, it is probable that the process must be conceived mainly in physiological terms.

[1] E. O. Finkenbinder, The curve of forgetting, *Amer. J. Psychol.*, 1913, 24, 21–23.

[2] K. M. Dallenbach & J. G. Jenkins, Oblivescence during sleep and waking, *ibid.*, 1924, 35, 605–612.

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